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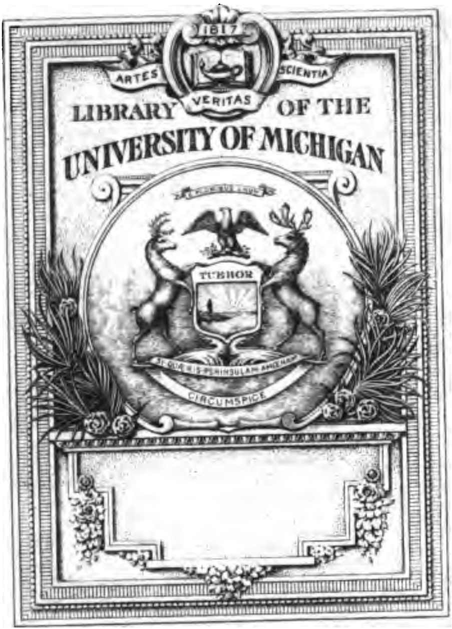
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Occasional Scientific Papers
of the
Westwood Astrophysical Observatory
Number I.

LUNAR AND TERRESTRIAL ALBEDOES

By
FRANK W. VERY

BOSTON: THE FOUR SEAS COMPANY

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1917

Astron. Obs.

HISTORICAL NOTE

FOUNDED in 1906, the Westwood Astrophysical Observatory owes its inception to aid from Percival Lowell. In beginning a special series of its publications, the writer wishes to place on record his indebtedness to the warm sympathy and encouragement of a faithful friend. Himself an ardent lover of freedom, Dr. Lowell never interfered with the writer's free and independent ordering of the researches conducted at Westwood, but with a rare disinterestedness he placed at his disposal numerous spectrograms taken at the Lowell Observatory by the skilful hands of Dr. V. M. Slipher for measurement with apparatus of the writer's design. Nevertheless, the gain was mutual, for the results throw unexpected light on some of Lowell's own researches and demonstrate that complete independence in respect to control and motives of action is not incompatible with a consistent working together for a common end.

Lowell had been greatly interested in the research which forms the subject of the present communication, with its obvious bearing on the problem of planetary temperature. In his "Temperature of Mars," he had adopted 0.75 for the albedo of a half clouded earth, and I, in my "Greenhouse Theory and Planetary Temperature," had taken 0.70 for the same datum, differing but little from the value now found for the geometrical albedo of the earth, which is 0.72.

Let me also place on record as a result of my intimate association with him, my recognition of the fact that his theories were based on an elaborate accumulation of unsurpassed evidence, that he was always open-minded to new evidence, and that, while presenting some revolutionary new conceptions, he did not hesitate to modify his own ideas when convinced that they could be improved. It is this willingness to revise that constitutes the true man of science. That there was very little for him to change as his researches progressed, is a testimony to Lowell's thoroughness and to his deep insight into nature's mysteries.

With gratitude to God for the gift of a friend—generous, thoughtful for others, and noble in his ideals, keenly critical, but kindly appreciative, learned, but modest—

I dedicate these researches

To the Memory of

Percival Lowell

THE WESTWOOD ASTROPHYSICAL OBSERVATORY

The Westwood Astrophysical Observatory is situated in Westwood, Massachusetts. Its approximate position and altitude (derived from the topographical map of the United States Geological Survey) are

Latitude = 42° 12′ 58″ North.
Longitude = 71° 11′ 58″ West.
Altitude = 190 feet above sea level.

Its publications hitherto have been in current scientific periodicals, especially, *Lowell Observatory Bulletin, American Journal of Science, Astrophysical Journal, Science, Astronomische Nachrichten,* and *Bulletin Astronomique.*

The Observatory possesses special instruments for the study of solar radiation and atmospheric transmission, for delicate heat measurements, the utilization of solar radiation and study of the "greenhouse" effect, photometry, spectral line and band comparator, etc. For several years it had the use of a fine silver-on-glass concave mirror of 12 inches aperture and 10 feet focal length, which was loaned by its maker, Dr. J. A. Brashear. The mirror was used in researches on the transmission of terrestrial radiation by the aqueous vapor of the atmosphere.

Special researches are being actively prosecuted on atmospheric transmission and the solar constant, quantitative measurements of the intensity of spectral lines, planetary atmospheres and temperatures, greenhouse theory, contributions to the theory of nebulæ and novæ, measurements of the earth's albedo and of that of the moon for all parts of the visible spectrum. The latter researches form the subject of the present communication.

LUNAR AND TERRESTRIAL ALBEDOES

Introduction.

THE word *albedo* (derived from the Latin *albus,* white) has been used by astronomers to designate the fraction of the sun's luminous rays reflected by a planet at full phase, allowance being made for the distances of the planet from sun and earth and for the dimensions of the reflecting body. If the planet were a smooth sphere with perfect specular reflection, it would be itself invisible, but would present within the diminutive limits of its disk a complete picture of the surrounding heavens, distorted by spherical aberration, but otherwise exact; and within this image the reflection of the sun would surpass in brilliancy all other objects, shining like a star at a point on the planet's disk distant from the center by the radius of the disk multiplied by the cosine of half the elongation of the planet from the sun. But whatever specular surfaces there may be on the planets of our solar system, they are of too limited extent to be recognized as such; and the planetary reflection of light is to be classed under the head of a generally diffusive one, though not necessarily an equable one in all directions; and in fact there are diversities in the distribution of the reflected light to different parts of the sphere which must be considered in getting the phase-curve of the illumination, and which are not entirely without influence even if we confine our attention to the reflection sent earthward at full phase, while they are vital to the determination of the complete reflection to the sphere.

Since all of the planets, except possibly some of the smaller asteroids, are spheroidal bodies, it is not necessary for purposes of intercomparison to refer their albedoes to the standard specific reflectivity of a flat surface; but it *is* desirable to distinguish clearly between the only thing which is certainly measurable in most cases,—which is (1) the geometrical albedo at full phase, or the amount of light sent earthwards at the planet's full phase, compared with that which would be sent by a sphere of the same size and at the same distance, which possesses perfect diffusive reflectivity;—and (2) that integration of the reflection to the entire sphere, or the *spherical* albedo, whose determination requires a knowledge of the phase-law. This law is very imperfectly known, except in the case of the moon, and hence there are rival hypotheses which give more than one kind of "spherical" albedo. There is even a diversity of usage in regard to what shall be called the "geometrical" albedo, although there need be no discrepancies in the facts of observation on which it is based. A very few words will suffice to make the fundamental distinctions plain as to their general principles; but the remoter consequences of the acceptance of the diverse points of view lead to discussions of some complexity whose complete unfolding can not be exhibited in the limits of this paper, but enough will be presented to give an intelligible conception of the subject.

If we measure the amount of light received by the eye from the full moon, that is to say, if we find the reflection of sunlight by a spheroidal surface to a point (since the pupil of the eye is virtually a point), we shall get the same value whether the moon is near the horizon or in the zenith (after correcting for the absorption by the earth's atmosphere); and it seems natural to take this constant light-quantity as the basis of *the geometrical albedo referred to a definite point in space,* comparing it with the quantity of light which would be given if the whole sky were filled with moons of perfectly diffusive reflecting quality, and viewed by turning the eye progressively to all parts of the sky and summing the successive impressions. This geometrical ratio of the *reflection to a point* compared with the perfectly diffuse reflection at that point from an ideal body of the same size and in the same situation, is the one considered in this paper and is what is meant by the geometrical albedo.

But if, instead of this, we take the *illumination of an extended surface* by the hypothetical sky full of moons, it is necessary to take into account the diminution of superficial illumination from those rays which are at low angles to the surface, and even supposing an absence of atmosphere, the *surface* illumination produced by a sky full of moons will only be half as great as the sum of the illuminations supposing each moon to be successively transported to the zenith. Thus the "surface illumination" is one half of the geometrical albedo.

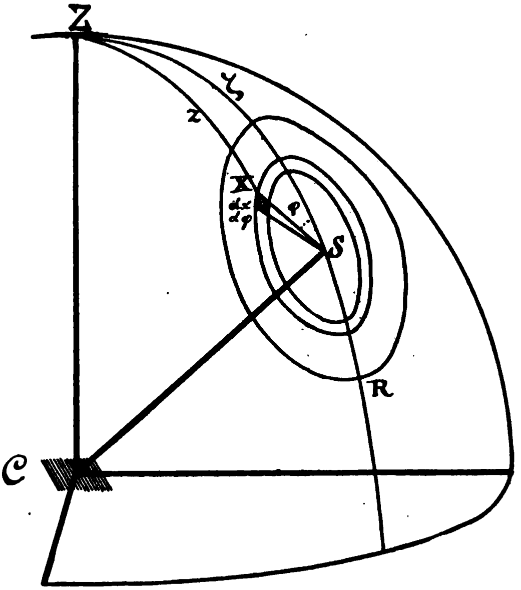

FIGURE I

Lambert showed in his *"Photometria"* (*cap.* II.) that if we seek the illuminating power *(L)* of a circular luminary of radius $SR = r$ (Fig. 1), whose center is at any point *(S)* of zenith-distance $SZ = \zeta$, upon a surface at *C*, we may obtain *L* by summing a series of annuli concentric with *S* and of radius $SX = x$, where, if an element $(dx, d\varphi)$ of the annulus is at the angle $ZSX = \varphi$ from the vertical through *S*, the area of the element is $dx \cdot d\varphi \sin x$. Hence

$$L = \int\int \sin x \cos \zeta \, d\varphi \, dx,$$

since the illumination of the surface at *C* varies in proportion to $\cos \zeta$.

By spherical trigonometry, if z is the zenith-distance of any point *X* on the annulus,

$$\cos z = \cos \zeta \cos x + \sin \zeta \sin x \cos \varphi,$$

and

$$L = \int\int dx \cdot d\varphi \sin x \,[\cos \zeta \cos x + \sin \zeta \sin x \cos \varphi]\,.$$

A first integration relatively to φ between $\varphi = 0°$ and $\varphi = 360°$, or 2π, gives

$$L = \int dx \sin x \cdot 2\pi \cdot \cos \zeta \cos x.$$

Integrating this with respect to x from $x = 0°$ to $x = r°$,

$$L = 2\pi \cos \zeta \int \sin x \cos x \, dx$$

$$= 2\pi \cos \zeta \left(\frac{1 - \cos 2r}{4}\right) = \pi \cdot \cos \zeta \cdot \sin^2 r.$$

This gives for the illuminating power *(L)* of the moon at the zenith to that of a sky full of moons *(L')* upon an extended surface at *C*, as a first approximation,

$$L : L' = \pi \cos 0° \; (\sin^2 15' \, 33'') \; : \pi \cos 0° \; (\sin^2 90°)$$
$$= 1 \; : 48{,}875.$$

But if we consider the luminous effect upon a point, such as the eye, or the heating effect upon the bulb of a thermometer which may likewise be taken as a point, instead of that communicated to an extended surface, then, neglecting atmospheric absorption, it is necessary to find the ratio of illuminations by taking the ratio of the area of the apparent lunar disk to the hemispherical sky area, a ratio which is half as great as the one just given. For a disk as small as that of a planet, the area may be taken $= \pi \sin^2 \varrho \cdot r^2$, where ϱ is the angular value of the radius of the disk and r is the distance of the planet. Comparing this with the area of the hemisphere, $2\pi r^2$, the latter exceeds the former in the ratio, $2 : \sin^2 \varrho$, which, for the moon's semi-diameter, $\varrho = 15' \, 33''$, gives for the ratio of the light reflected to a point from the two sources,

$$97{,}750 \; : 1$$

with a similar degree of approximation to the preceding value.

The integration of the total light reflected by the illuminated hemisphere of the planet in all directions requires the introduction of hypotheses. The first is Lambert's hypothesis of uniform diffuse reflection, of which the following account is substantially that of Müller.

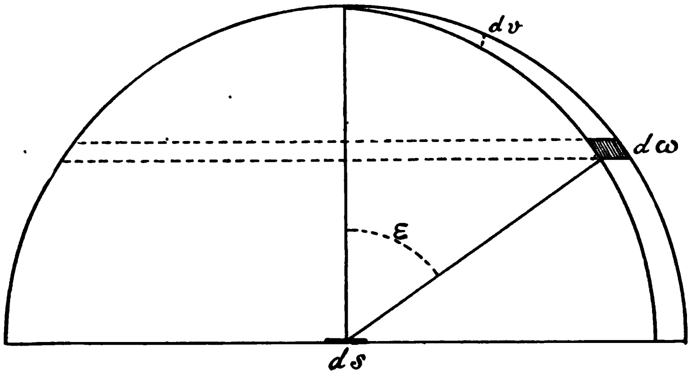

FIGURE 2

Consider a diffusely reflecting surface-element, *ds* (Fig. 2) illuminated under any angle of incidence, *i*. If *L* is the quantity of light which falls normally on the unit of surface, then *ds* receives *L ds* cos *i*, of which a certain fracton *cL ds* cos *i* is reflected normally, and in any other direction, such as that of the emanation angle ε, the light-quantity $dq = cL \cos i \, ds \cos \varepsilon$ is reflected, ***provided the surface reflects as well at one angle as at another.***

Construct a hemisphere with radius 1 about *ds* of which an element *dω* receives the fractional light-quantity

$$dQ = dq \, d\omega = cL \, ds \cos i \cos \varepsilon \, d\omega.$$

Then since the element *dω* has the width *dv* sin ε and the height *d*ε, or the angular area *d*ε sin ε *dv,* the total light-quantity has the value

$$Q = cL \, ds \cos i \int_0^{\pi/2} \cos \varepsilon \sin \varepsilon \, d\varepsilon \int_0^{2\pi} dv,$$

and since $\int_0^{\pi/2} \cos\varepsilon \sin\varepsilon \, d\varepsilon = \tfrac{1}{2}$, and $\int_0^{2\pi} dv = 2\pi$,

$$Q = L\, ds \cos i \cdot A,$$

where the factor A is a fraction which tells how much of the incoming light is reflected to a hemisphere of radius 1. A, which is always smaller than 1, is simply called the albedo of the substance by Lambert.

From the two equations for Q, it follows that $c = A/\pi$, and thence we have Lambert's law of illumination by diffusely reflecting substances in the well known form:

$$dq = \frac{A}{\pi} L\, ds \cos i \cos \varepsilon,$$

or

$$dq = \Gamma_1\, ds \cos i \cos \varepsilon$$

if $\Gamma_1 = AL/\pi$.

Calculation of the quantity of light sent to the earth at different phases of a reflecting planet requires some further slight modifications.

Let a plane be drawn through the middle point of the planet at right angles to the earth-planet line; and let its intersection with the planet's surface be represented by the circle $ABCD$ (Fig. 3). The perpendicular to this plane in the direction of the earth is shown diagrammatically by ME. MS is drawn in the directon of the sun. The arc of a great circle ES is the phase-angle α, taken from full phase. An element ds of the visible hemisphere of the planet is connected with E and S by great circles of the sphere. Arc $S - ds = i$, arc $E - ds = \varepsilon$. Latitude of $ds = \psi = F - ds$. Longitude from $E = EF = \omega$.

From the right-angled spherical triangles $FSds$ and $FEds$, we have the relations:

$$\cos i = \cos \psi \cos (\omega - \alpha),$$

$$\cos \varepsilon = \cos \psi \cos \omega.$$

If the semi-diameter of the planet is ϱ, the linear dimensions of ds are $\varrho\, d\psi$ in the direction of the meridian and $\varrho\, d\omega \cos \psi$ along a parallel. Hence

$$\text{surface of } ds = \varrho^2 \cos \psi \, d\omega \, d\psi.$$

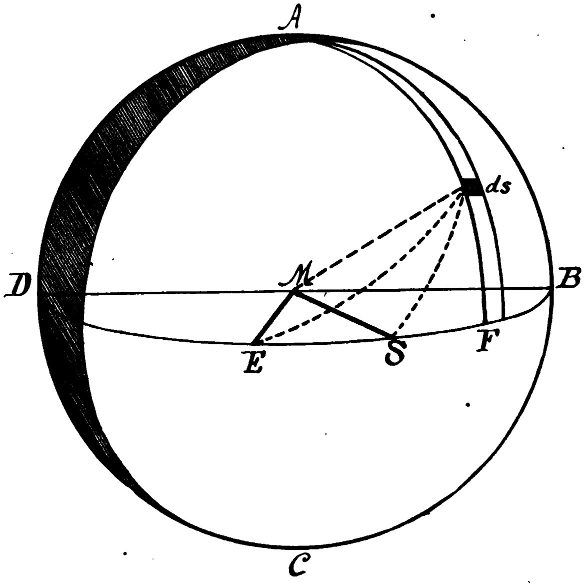

FIGURE 3

We now introduce two rival hypotheses, or laws of reflection, (1) that reflection is uniform in all directions, (2) that it varies according to a definite law, and get

$$(1)\begin{cases}\text{Lambert's Law:}\\ \text{1a.}\quad dq_1 = \Gamma_1\, \varrho^2 \cos^3 \psi.\, d\psi \cos(\omega - \alpha) \cos \omega\, d\omega,\\ \text{Lommel-Seeliger Law:}\\ \text{1b.}\quad dq_2 = \Gamma_2\, \varrho^2 \cos^2 \psi \dfrac{\cos \omega \cos(\omega - \alpha)}{\cos(\omega - \alpha) + \lambda \cos \omega} d\omega,\end{cases}$$

where $\Gamma_2 = \frac{L}{4\pi} \frac{\mu}{k}$ and $\lambda = \frac{k}{k_1}$, k being the coefficient of absorption of the rays which enter into the interior of the substance of the planet's surface, k_1 the coefficient of interior absorption of the returning rays on the way to emission, and μ the diffusive power of the body. In general $k_1 < k$, because the outgoing rays have lost their more absorbable ingredients. If the material is strongly colored, k may be very much larger than k_1.

The derivation of the Lommel-Seeliger equation which takes account of interior reflection and diffusion is very complicated. The final equation is

$$dq_2 = \frac{\mu L}{4\pi k} \cdot ds \cdot \frac{\cos i \cos \varepsilon}{\cos i + \lambda \cos \varepsilon}.$$

Confining attention here to the Lambert equation, the formula must be integrated over that part of the illuminated surface visible from the earth. The integration limits for ψ are $-\pi/2$ and $+\pi/2$, and those for ω are $-\pi/2+\alpha$ and $+\pi/2$,

whence $q_1 = \Gamma_1 \varrho^2 \int_{-\pi/2}^{\pi/2} \cos^3 \psi \, d\psi \int_{\alpha-\pi/2}^{\pi/2} \cos(\omega - \alpha) \cos \omega \, d\omega.$

But

$$\int_{-\pi/2}^{\pi/2} \cos^3 \psi \, d\psi = \int_{-\pi/2}^{\pi/2} \cos^2 \psi \, d(\sin \psi)$$

$$= \int_{-\pi/2}^{\pi/2} [1 - \sin^2 \psi] \, d(\sin \psi) = \frac{4}{3}.$$

And

$$\int_{\alpha-\pi/2}^{\pi/2} \cos(\omega - \alpha) \cos \omega \, d\omega$$

$$= \tfrac{1}{2} \int_{-\pi/2}^{\pi/2} \cos \alpha \, d\omega + \tfrac{1}{2} \int_{\alpha-\pi/2}^{\pi/2} \cos(2\omega - \alpha) \, d\omega$$

$$= \tfrac{1}{2} [(\pi - \alpha) \cos \alpha + \sin \alpha].$$

Therefore

$$q_1 = \Gamma_1 \varrho^2 \cdot 2/3 \, [\sin \alpha + (\pi - \alpha) \cos \alpha]. \tag{2}$$

For the full phase, when sun, earth and planet stand in a straight line, $\alpha = 0$ and the reflected light is $q_1^{(0)} = \Gamma_1 \varrho^2 \cdot 2/3\pi$. Hence we have for the ratio

Light at phase-angle α : Light at full phase,

$$q_1/q_1^{(0)} = \frac{1}{\pi} [\sin \alpha + (\pi - \alpha) \cos \alpha]. \tag{3}$$

A similar integration for the Lommel-Seeliger law gives

$$(4)\qquad q^2 = \frac{\Gamma_2 \varrho^2 \pi}{2}[1 - \sin \alpha/2 \tan \alpha/2 \log \cot \alpha/4].$$

But for $\alpha = 0$, $q_2^{(0)} = (\Gamma_2 \varrho^2 \pi)/2$, and

$$(5)\qquad q_2/q_2^{(0)} = 1 - \sin \alpha/2 \tan \alpha/2 \log \cot \alpha/4.$$

The distribution of light over the apparent disk of the planet varies according to the adopted law. Euler's law would demand uniform light, except for a narrow strip of sudden diminution at the terminator and an excessively narrow, but exceedingly bright rim at the illuminated limb. Nothing of the sort is observed, and this law may be dismissed at once. Moreover, Euler considered nothing but superficial reflection, just as Bouguer did, whereas the penetration of the light into a thin surface layer, even in the most opaque substances, is of great importance.

Lambert's law appears to work fairly well where the reflecting medium is of the nature of cloud with internal diffusion and multiple reflection from innumerable widely dispersed and finely divided particles, such as ice crystals, or dust, or the liquid water particles of ordinary cloud. The Lommel-Seeliger Law is more appropriate for extended solid surfaces at various inclinations to the incident light. A composite inter-mingling of solid surface and cloud requires a mixture of the two laws.

In the following Table are given the computed values of the functions of the phase-angle, $\varphi(\alpha)$, for intervals of 5° according to several theories. These quantities are then multiplied by others proportional to the areas of the corresponding zones, $\sin \alpha \cdot \Delta\alpha = \Delta(1 - \cos \alpha)$, to give the values in the last three columns which, being summed, produce the proportional factors for the spherical albedo. If the intervals had been taken small enough, the sum of the differences of versine α, $\Sigma [\Delta(1 - \cos \alpha)]$, would have been exactly 2 which, multiplied by 2π, gives the area of the sphere of unit radius. The average reflection by a planet to the sphere has an intensity ¼ if the reflection is perfect (normal specific reflectivity $= 1$), this being the mean between the quantities (½ and 0) sent in the directions of source and antipodal point. Calling $\Sigma [\Delta(1 - \cos \alpha) \cdot \varphi(\alpha)]$ the *spherical factor,* its values given in the last line of the table, are

0.75 by Lambert's Law.
1.50 by the Lommel-Seeliger Law.
0.35 by the lunar phase-curve.

While the spherical albedo can not exceed unity, there may be various distributions of light to the sphere. Thus, for perfect reflection, the diverse spherical factors obtained from the summations in the table are consistent with geometrical albedoes of 0.50, 1.33, 0.67, and 2.86, the last being for the phase-law of the moon where the reflection at full phase is extraordinarily large.

Limits of phase-angle $\alpha_1 - \alpha_2$	$\Delta(1-\cos\alpha)$	$\varphi(\alpha)$ Lambert	$\varphi(\alpha)$ Lommel-Seeliger	$\varphi(\alpha)$ Moon	$\varphi(\alpha) \times \Delta(1-\cos\alpha)$		
					Lambert	Lommel-Seeliger	Moon
0°— 5°	.0038	.996	.999	.957	.0038	.0038	.0036
5 — 10	.0114	.986	.994	.869	.0112	.0113	.0099
10 — 15	.0189	.972	.985	.783	.0184	.0186	.0148
15 — 20	.0262	.953	.974	.703	.0250	.0255	.0184
20 — 25	.0334	.928	.961	.628	.0310	.0321	.0210
25 — 30	.0403	.898	.947	.557	.0362	.0382	.0224
30 — 35	.0469	.863	.931	.494	.0405	.0437	.0232
35 — 40	.0531	.824	.915	.440	.0438	.0486	.0234
40 — 45	.0589	.781	.898	.392	.0460	.0529	.0231
45 — 50	.0643	.734	.880	.348	.0472	.0566	.0224
50 — 55	.0692	.684	.862	.310	.0473	.0597	.0215
55 — 60	.0736	.633	.844	.275	.0466	.0621	.0202
60 — 65	.0774	.581	.826	.243	.0450	.0639	.0188
65 — 70	.0806	.531	.808	.213	.0428	.0651	.0172
70 — 75	.0832	.481	.790	.185	.0400	.0657	.0154
75 — 80	.0852	.434	.772	.159	.0370	.0658	.0135
80 — 85	.0865	.388	.755	.136	.0336	.0653	.0118
85 — 90	.0872	.342	.738	.115	.0298	.0644	.0100
90 — 95	.0872	.298	.721	.096	.0260	.0629	.0084
95 —100	.0865	.256	.705	.080	.0221	.0610	.0069
100 —105	.0852	.218	.690	.066	.0186	.0588	.0056
105 —110	.0832	.183	.675	.054	.0152	.0562	.0045
110 —115	.0806	.150	.661	.044	.0121	.0533	.0035
115 —120	.0774	.120	.648	.035	.0093	.0502	.0027
120 —125	.0736	.094	.636	.028	.0069	.0468	.0021
125 —130	.0692	.073	.625	.021	.0050	.0433	.0015
130 —135	.0643	.055	.614	.016	.0035	.0395	.0010
135 —140	.0589	.040	.605	.012	.0024	.0356	.0007
140 —145	.0531	.028	.596	.010	.0015	.0316	.0005
145 —150	.0469	.019	.589	.008	.0009	.0276	.0004
150 —155	.0403	.015	.582	.006	.0006	.0235	.0002
155 —160	.0334	.012	.577	.005	.0004	.0193	.0002
160 —165	.0262	.010	.572	.005	.0003	.0150	.0001
165 —170	.0189	.008	.569	.004	.0002	.0108	.0001
170 —175	.0114	.006	.567	.004	.0001	.0065	.0001
175 —180	.0038	.004	.566	.004	.0000	.0022	.0000
Sums	2.0202				0.7503	1.4874	0.3491

LUNAR AND TERRESTRIAL ALBEDOES

DISCUSSION OF THE OBSERVATIONS[1]

In the *Astrophysical Journal* for April, 1916, Professor H. N. Russell has discussed some recent observations of the writer on the earth-shine, from which the earth's albedo had been obtained indirectly. The observations are of two sorts—(1) direct visual comparisons of parts of the moon (lit by the sun's rays) and of other parts of similar quality, lit by the earth-shine, with the light of a flame seen through blue glass; and (2) comparisons of the relative intensities of all the colors in the spectrum between violet and red from their relative photographic effect on spectrograms.

The earth-shine spectrograms,[2] along with similar ones of the moon and of the sky, were measured at the Westwood Observatory by means of the comparator originally designed for quantitative measures of the intensities of atmospheric bands on the Lowell Observatory spectrograms of Mars and the Moon. With this instrument I have already obtained approximate determinations of the amounts of the water vapor and oxygen in the atmosphere of Mars, and I am now engaged in measuring the intensities of the Fraunhofer lines in the solar spectrum with an accuracy which has not been approached hitherto. These facts show that on the score of precision, the comparator is capable of excellent work, though, like all instruments dependent on photography for the registration of intensities, it involves the complexities of photographic laws. These complexities, however, I have endeavored to minimize, and I have in large part succeeded in eliminating them by an extensive study of the photographic problem for each wave-length and a wide range of exposures on more than one kind of plate. Professor Russell's criticisms of my work with the spectral line and band comparator are largely founded on misapprehensions. He has taken unwarrantable liberties with my figures and by so doing has rejected my work on the spectrograms on insufficient grounds. When correctly reduced, the two methods give results which are in good agreement, but on the whole, those from the spectrograms are the more reliable.[1]

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[1] The essential features of this discussion were presented before the American Astronomical Society at its Nineteenth Meeting at Swarthmore, Pennsylvania, August 31, 1916.

[2] The spectrograms were made for me through the kindness of Dr. Percival Lowell by Dr. V. M. Slipher at Flagstaff. Several of them were taken under exceptionally favorable atmospheric conditions. See Frank W. Very—"The Photographic Spectrography of the Earth-Shine," *Astronomische Nachrichten*, Nr. 4819-20, Bd. 201, s. 353-400, November, 1915; also "Atmospheric Transmission," *Science*, N. S., Vol. XLIV., No. 1127, pages 168-171, August 4, 1916.

In his first article,[2] Russell adopts 465,000 : 1 for the ratio of sunlight to full-moon light; and on page 184 of the April *Journal* he expresses preference for the ratio 9,000 : 1 [3] between sunlight and full-earth light on the moon. According to this, the ratio of full-earth light to full-moon light is

$$465{,}000 : 9{,}000 = 51.7 : 1,$$

[1] The visual photometric values of the earth-shine which are described in my paper on "The Earth's Albedo" (*Astronomische Nachrichten*, Nr. 4696, Bd. 196, s. 269-290, November, 1913) were obtained with a special earth-shine photometer which might be improved in the light of the experience gained with it. Although free from photographic difficulties, the method has difficulties of its own, as may be recognized from the elaborate researches which were required in establishing the constants of the various absorbent pieces. Owing to the faintness of the earth-shine, the low altitude of the crescent moon when the measures have to be made, and the varying transparency of the atmosphere, there are further difficulties which Professor Russell generously allows for in his criticism, but he has not understood some of the minor details.

It must be remembered that the spectrograms were made with an analyzing spectroscope, and that the values obtained relate to the intrinsic brightness of definite regions on the moon where the reflecting quality of the surface is far from uniform, and the range of luminous values with the phase is wide, so that small displacements on the surface may give considerable alteration of light. The observed differences are due to these unavoidable vicissitudes, rather than to errors of observation; but such differences as these tend to average out from the general mean. In the earth-shine exposures, the slit was placed half on and half outside the dark limb of the moon to give the sky spectrum needed in the reductions.

[2] *Astrophysical Journal* for March, 1916, p. 125.

[3] On page 194 (*op. cit.*), this ratio is attributed to me, but I have not given it, and prefer the ratio 10,000 : 1.

which, since the angular area of the earth as seen from the moon is 13.4 times that of the moon seen from the earth, makes the earth's albedo 51.7/13.4 = 3.86 times that of the moon. My own determination of this ratio is considerably larger, namely, 4.8 : 1.

The ratio of sunlight to moonlight is not easily measured with precision on account of the wide range of intensities involved and the uncertainties of atmospheric absorption. Whether we take the ratio 465,000 : 1, difference of magnitude = 14.17 (Russell), or 618,000 : 1, difference of magnitude = 14.48 (Zöllner), or even a value as large as Wollaston's (801,000 : 1) we shall still be inside the actual divergences of some very good observers. A critical examination of sources of error will improve this result greatly.

Zöllner's explanation of the peculiar efficacy of the lunar mountains in emphasizing the peak of the lunar phase-curve at the full, with Searle's emendation which notes the contribution to the same effect by crevices which retain the sunshine away from observation until the short interval when the rays strike their floors, suffices for the anomalies of this phase-curve. Whether the elevations are mountains, as is usually assumed, or innumerable crystalline facets, that is, whether the roughness is on a large or a small scale, is a matter of indifference. Zöllner's diagrammatic figure is well characterized by Russell as "artificial," but the fact of a general excessive roughness of the lunar surface is probable enough and natural enough, even though it may not be as obvious as the mountains are to telescopic vision. Nevertheless, although Zöllner's hypothetical moon behaves in some respects like the actual moon near the time of full, the analogy would fail if pushed to its limit, especially in the early crescent phases, and the title "true" which was used by Zöllner to designate the "albedo" of this hypothetical body is a misnomer when applied to the actual moon. Russell's criticism by questioning the foundation of an almost unanimous acceptance of Zöllner's value and terminology, performs a much needed use. In this he has followed Guthnick more or less. Müller, also, had previously characterized Zöllner's value of the mean (52°) slope of the lunar upheavals as "illusory."

In getting the lunar albedo, Zöllner allowed for the rotundity of the moon, and by an unfortunate misapplication of Lambert's law of reflection from a uniformly diffusing sphere[1] found that the full moon should reflect

$$\frac{1}{p} = \frac{1}{(3/2) \times 48{,}980} = \frac{1}{73{,}470}$$

of sunlight, if it were such a sphere. The corresponding value of the albedo he called the "apparent albedo" (scheinbare Albedo)—a term which is misleading, since it implies that the moon's reflection which is actually observed is this quantity given by computation, and that the reflection up to this point follows Lambert's law, which is not true.

The *total* radiation of the moon, including both reflected and emitted rays, does appear to follow pretty nearly a sequence which can be derived from Lambert's law, multiplied by the factor 2/3, a number which seems to turn up on every hand in this research. This may be seen from the phase-curve given in my "Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase," where I found

At first quarter, total radiation = 17.7% of radiation from full moon.
At last quarter, total radiation = 24.8% of radiation from full moon.
Mean (quadrature) total radiation = 21.25% of radiation from full moon.
Lambert's law, multiplied by 2/3 = 21.22% of radiation from full moon.

Here the radiation which is measured, is made up of two parts—one which is wholly reflected, and the other an emission from a heated surface whose temperature varies from a maximum at the center of the disk to a minimum at the limb, while the surfaces of equal temperature are concentric zones. The *luminous* reflection, at any rate, follows the opposite law and is greater at the limb, and probably the non-luminous rays are reflected in the same way. The smaller total radiation at first quarter is of course due to the fact that the moon is getting hotter and the energy is being expended in modifying the subsurface thermal gradient, while the heat thus retained is given out again in the lunar afternoon, or at last quarter. The point which I wish to make clear is that, as far as the reflection of the moon's *light* is concerned, the introduction of Lambert's law at this or any other stage of the computation was a mistake.

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[1] The law was designed to give the *spherical* albedo. What was needed here was simply the geometrical albedo.

The value "$\mu = 0.1195$," which Zöllner calls "die scheinbare Albedo des Mondes," is a hypothetical value which is not immediately given by observation, but is obtained by restricting the definition of albedo to diffuse reflection from a nonexistent smooth sphere on the supposition that a factor 2/3 must be introduced. I will return to this later, but will note here that the moon does not reflect much after the approved fashion of a sphere, but acts, to all appearance, more like a flat surface, or even like one a little dished at the margin; for whereas a diffusive sphere should send out light from the marginal zones at full into a rear hemisphere, whereby these zones as seen from the front should be considerably fainter than the center, it is found, on the contrary, that the limb in the actual moon is in fact the brighter of the two. Both front and rear reflections from the limb are exceptionally large, so that these portions of the lunar surface are but little heated by the sun's rays.

The arguments by which Zöllner persuaded himself that he had arrived at the "true" value of the moon's albedo from his "apparent" albedo are somewhat involved. One of the first needs is that our procedures and definitions may be clarified and simplified. Let us begin by considering the Lambert law.

On page 176 of his second article, Professor Russell says correctly in speaking of the reflective function of the phase-angle, $\varphi(\alpha)$: "The whole amount of light reflected by the planet to the celestial sphere will be proportional to

$$\int_0^\pi \varphi(\alpha) \sin \alpha \, d\alpha.$$

If it shone in all directions with the brightness of the full phase, the emitted light would be 2.0 on the same scale." But this being so, the factor q which transforms from the geometrical albedo at opposition to the spherical albedo should be the integral just given, and not twice that quantity, which is Russell's equation (7), because the value of the integral alone without the coefficient 2 is 2.0, if $\varphi(\alpha)=1$ everywhere. For Lambert's law,

$$(6) \qquad q=\int_0^\pi \varphi(\alpha) \sin \alpha \, d\alpha=0.75,$$

and this is the Lambert factor for spherical albedo, or it is the spherical albedo if the geometrical albedo at full phase is unity, instead of $q=1.5$ as given by Russell. Similarly, for the lunar phase-curve, $q=0.35$, which is to be multiplied into the observed geometrical albedo from the full moon to give the lunar spherical albedo. Since the values of q have been taken two times too large, all of the numbers in Russell's Table I., *op. cit.*, page 179, should be divided by two. On the other hand, in getting q for the Lommel-Seeliger law, Russell has inconsistently dropped the factor 2, which would make his value the same as mine, were it not that there is a further inaccuracy in the integration by which he gets "$q=1.6366$." His equation (7) should give $q=3.0$.

Lambert's formula for spherical albedo,

$$L=(1/\pi)\ [\sin \alpha-\alpha \cos \alpha]+\cos \alpha,$$

where α is the moon's elongation, or phase-angle from conjunction, or as Russell prefers to put it, employing *phase-angles from opposition,* (in which respect I shall follow his procedure)

$$\varphi(\alpha)=(1/\pi)\ [\sin \alpha+(\pi-\alpha) \cos \alpha],$$

gives the light at quadrature, $L_{90}=1/\pi=0.318$, when the light at the full phase is unity. For the emission of its own radiation combined with the reflection, the radiant observation already quoted would seem to favor the value, $R_{90}=1/(1.5\pi)=0.212$. Here, however, we must note that, though the emission may be independent of its direction, the distribution of temperature is not uniform, and the result is a complex of two different functions of α.

Under these circumstances we can attach little significance to this special value. It is difficult to see how a particular numerical factor can survive this double vicissitude. If the factor 2/3 occurs undisguised in both functions, it will become 4/9 in their product; if in opposite senses, it will cancel out;

and if the factor is found in one case, but a different one is substituted for it in the other, it could not be so easily recognized.
By Lambert's law, at the full, $L_0 = \pi \times L_{90} = 3.14\ L_{90}$.
For the planet Venus, reflection $= L_0 = (1/.283)\ L_{90} = 3.53 L_{90}$.
Lunar emitted and reflected radiation $= R_0 = (3/2)\pi \times R_{90} = 4.71 R_{90}$.
Lunar reflected light $= L_0 = (1/.105)\ L_{90} = 9.52 L_{90}$.

The factors connecting the observed light at full phase with the light at quadrature are evidently empirical. Those connecting spherical and geometrical albedoes are probably equally empirical. The appearance of the factor $(3/2)\ \pi$ in my lunar radiation observation, to which I have called attention, is rather striking, yet it is probably no better than a coincidence. Zöllner must have been under a great misapprehension when he attempted to introduce the factor 3/2 into the discussion of his lunar observations, for it does not fit the facts.

There is, it is true, a universal usage for which the factor 3/2 is appropriate, namely: The reflection from a sphere of any ordinarily diffusive material is 2/3 of that returned with perpendicular incidence and reflection from a plane surface of the same substance, and in comparing the reflection of the entire spherical body of a planet with that from a plane surface of some terrestrial substance, it would be appropriate, provided we could be sure that the light is diffusively reflected, to multiply the reflection from the sphere by 3/2 to put it on terms of equality with the recognized reflective power, or specific reflectivity, of the given terrestrial material in the form of a flat surface when this is viewed normally. If the flat surface reflects the light at an angle of 45°, its reflection must be multiplied by $\cos 45° = 0.707$, and in this case the two reflectors are already on terms of approximate equality. It does not appear that the factor 3/2 was introduced by Zöllner with any such end as this in view, but simply because it occurs in the Lambert formula. As applied by Zöllner in his lunar theory, this use of the factor 3/2 has been a stumbling block in the path of subsequent research on the subject. The whole of this cloudy lucubration should be swept aside. Zöllner's original lunar observations are among the best that have ever been made, and they deserve to be rescued from the scandalous treatment of his theoretical discussion.

At the start, Zöllner has evidently adopted the incorrect idea that the fraction of light from the sun received upon the moon's surface and which has to be considered, is the fraction of the *total* luminous output of the sun to the entire sphere, and he thus gets for the denominator the number 48,980. He sees that this number is too small for his observations and makes the hypothesis that it must be multiplied by 3/2, giving 73,470, to which he assigned the symbol p. But this is in turn too small, and he introduces the further hypothesis of the lunar mountains with slopes of 52°, getting a new factor $\varkappa$, and $\varkappa p = 107{,}300$, with which his value of the so-called "true" albedo, 0.174, was obtained by comparing it with the observed ratio of sunlight to moonlight. If he had started out with the correct conception that the moon receives a certain fraction of the light emitted by the *half* sphere of the sun which is turned toward the planet, he would have obtained at once the number 97,960 (slightly different from the one which I have used, because we have adopted slightly different values of the moon's semidiameter) and he would have obtained at once for the geometrical albedo of the moon, $97{,}960/618{,}000 = 0.1585$.

In short, Zöllner started with a wrong number, multiplied this by 3/2 and then by nearly another 3/2, or in all by nearly 2¼, instead of by 2 exactly, as he should have done, and thus by a threefold error he reached a result which was nearly right, but solely by accident.

Russell starts with the same erroneous conception that the sun's complete spherical emission should be considered, but immediately abandons it (though without noting the fact) for another, not necessarily incorrect, but different from mine, since he does not introduce 4 into the numerator of his expression for p, nor yet the number 2 which would give what I call the geometrical albedo, but multiplies by 1, whence his p is one half of the geometrical albedo, given by eye observation, and represents *surface illumination* as I have shown in the Introduction. He then makes the reverse change by introducing 2 into his value of q, outside the integral, where it does not belong if by q is meant the spherical factor, as in my equation (6), so that his q is two times mine and through the cancellation of these opposite transformations we finally reach similar results for the spherical albedo.

The geometrical albedo, A_2, which, like Russell's p, "depends only on the geometrical and photometric relations of the planet as observed at the full phase"[1] is correctly given by equation (7),

$$A_2 = \frac{2M_0 \sin^2 S}{\sin^2 s_0 \sin^2 \sigma_0}, \tag{7}$$

where S is the apparent semi-diameter of the sun as seen from the earth, s_0 and σ_0 are the semi-diameters of the sun as seen from the planet and of the planet as seen from the earth at the time of opposition, and M_0 is the ratio of the light received from the planet at mean opposition, to the light of the sun as observed from the earth. This equation is the same as the middle one of Müller's (14)[2] and is a special case derived from the Lommel-Seeliger theory, which, although it is a theory of spherical reflection, gives the geometrical albedo at this particular point,[3] which Russell's equation (5) does not do.

Professor Russell says: "Let r be the mean radius of the planet's disk, and R its distance from the sun, and M_0 be the ratio of the apparent brightness of the planet at the full phase, and at distance Δ from the earth, to that of the sun at unit distance. The fraction of the sun's whole radiation which the planet intercepts is $r^2/4R^2$."[4] This is all true, but it is not what we want to know. The fraction of the sun's light emitted by the solar hemisphere which is visible from the planet, and which the planet intercepts, is $\pi r^2/2\pi R^2 = r^2/2R^2$; and M_0 being the ratio of the observed planetary light at full phase to sunlight and Δ the distance from the earth at the time of observation, so that if the other things remain the same, $M_0\Delta^2 =$ const., an alternative expression for the geometrical albedo is

$$A_2 = M_0\Delta^2 / \frac{r^2}{2R^2} = \frac{2R^2 M_0 \Delta^2}{r^2}.$$

It is not necessary in this problem of the reflection of the sun's rays to consider the sun's invisible hemisphere. The reflection would be the same if this were dark. For the particular problem under discussion, the other side of the sun is as if it were non-existent, and it has nothing to do with the question. That there may be no misunderstanding, I repeat that Russell's final expression for what he denotes under the symbol p, and which I call surface illumination, must be multiplied by 2 in order to obtain the quantity A_2 or the geometrical albedo; and therefore his factor "q" which reduces to spherical albedo is to be divided by 2, so that, apart from all other considerations, that is, granting the reliability of the original data, there should be no difference between us in respect to the spherical albedoes ("A" of Russell's Table V), provided we could agree in regard to the best phase-law to be adopted.[1]

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[1] H. N. Russell, in *Astrophysical Journal* for April, 1916, p. 177.

[2] *Photometrie der Gestirne*, p. 65.

[3] The geometrical albedo is in fact the same as the spherical albedo on the assumption that the spherical factor is unity ($q = 1$).

[4] *Astrophysical Journal*, April, 1916, p. 176.

The quantity "p" is defined in two ways in Russell's paper:

"The factor p may also be defined as the ratio of the actual brightness of the planet at the full phase to that of a self-luminous body of the same size and position, which radiates as much light from each unit of its surface as the planet receives from the sun under normal illumination" (*Op. cit.*, pp. 177-178).

By this definition, as interpreted by Russell, the quantity p is proportional to Müller's M_0, since all of the planetary values in Russell's equation have been reduced to unit distance, and M_0 is a ratio to sunshine at unit distance. But from (7)

$$M_0 = \tfrac{1}{2}A_2 \times const.,$$

or Russell's p, like Müller's M_0, is proportional to the half of the geometrical albedo.

The writer would interpret the definition itself differently, because a planet of radius r and distance from the sun R, will receive from the sun, if L is the sun's total spherical emission of light, the light-quantity $L/4\pi R^2$ on each unit of normally exposed surface. But the planet "receives" light from the sun on only one half of the planetary surface, and hence, if it radiates from "each *unit* of its surface" self-luminously (or what amounts to the same thing, if the light falling normally on a plane surface equal to the planet's section, is wholly emitted by a hemispherical surface of that planet) the emitted light is given out through a surface twice as great as the receiving surface, and should be on the average intrinsically half as intense as the received light. If the intrinsic brightness of the emitted light is

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[1] **This will be considered in a separate paper.**

$$J = \tfrac{1}{2}(L/4\pi R^2),$$

the total light emitted by the sphere is

$$L' = 4\pi J r^2 = \frac{L}{2}\frac{r^2}{R^2}.$$

But this does not represent the real conditions, because the definition itself is faulty, since the planet does not receive light "under normal illumination" on every part of its exposed hemisphere. If, therefore, we make my parenthetical substitution, and omit the stipulation that each unit of surface shall radiate as much light as it receives "under normal illumination," the total light emitted by the *hemisphere* is

$$L' = 2\pi J r^2 = \frac{\mathrm{L}}{4}\frac{r^2}{\mathrm{R}^2},$$

and we have returned to the previous proposition relating to the fraction of the sun's whole spherical emission which the planet intercepts. This, as I have said, does not concern us in the problem of reflection, where the planet reflects light solely from the visible hemisphere of the sun, and the invisible solar hemisphere with its emission need not be considered, since none of its light reaches the planet.

We have, therefore, for the reflection-ratio, if all of the sunlight is reflected,

$$L'/\frac{L}{2} = 2\frac{L'}{L} = \frac{r^2}{2R^2},$$

and the geometrical albedo is the ratio of the observed reflection to this value; whence Russell's first definition, if modified so as to bring it into accord with the actual conditions, agrees with my definition of the geometrical albedo.

On the other hand, Russell's second definition can not be thus reconciled. It reads as follows:

"The factor p may be defined verbally as the ratio of the observed brightness of the planet at full phase to that of a flat disk of the same size and in the same position, illuminated and viewed normally, and reflecting all the incident light in accordance with Lambert's law" (*op. cit.*, p. 188).

If the sphere shines "in all directions with the brightness of the full phase" (*op. cit.*, p. 176), the quantity which I call q (eq. 6) is

$$q = \int_0^\pi \varphi(\alpha) \sin \alpha \, d\alpha = 2.0.$$

But if the sphere shines according to Lambert's law, the value of the integral is 3/4. If, however, the further proviso be made that p is "the ratio of the observed brightness of the planet at full phase to that of a flat disk," since the ratio of reflection from a sphere illuminated and viewed from the front (or that reflection corresponding to its geometrical albedo), is to the reflection from a flat disk, illuminated and viewed normally, as 2/3 : 1, the 3/4 must be multiplied by 2/3 giving 1/2 as the maximum "flat-disk" value of the spherical albedo. The same fraction expresses the relation between the intregral computed for $\varphi(\alpha)$ as given by Lambert's law (q_1) and by the Lommel-Seeliger law (q_2), namely,

$$q_1 : q_2 = 0.75 : 1.50 = 1 : 2.$$

Since, however, the total reflection, or spherical albedo, must be the same and equal to the incident light on either hypothesis *with complete and diffusive reflection,* it follows that whatever differences result from these hypotheses must fall upon q and p equably and oppositely in order that their product, $A = qp$, may remain the same for the given planet. That is to say, whatever variations there may be in the distribution of light to different zones by reflection according to the rival theories, the sum total, or spherical albedo, must be the same for either if the reflection is complete. Hence if q is obtained by Lambert's law, p must be twice as large as it would be if q were given by the Lommel-Seeliger law. There appears, therefore, to be an inconsistency in Russell's second definition, and his "p" matches the condition demanded by the Lommel-Seeliger law, while my doubled value, though given as a particular case of the Lommel-Seeliger formula, has to be combined with the Lambert value of q, if $A = qp$ is to be kept constant. We thus reach the curious dilemma that neither of these two rival theories can dispense with the other. As if to enforce this point, the actual phase-curve of Venus follows a mixture of the two laws. It becomes exceedingly difficult to "mind your *p*'s and *q*'s," under these circumstances.

If we knew the mean temperature of a planet for all latitudes from the equator to the poles, we should no doubt find some relation between the thermal quantity corresponding to this temperature and the spherical albedo of the planet. In general, we may anticipate that the greater the spherical albedo is, the less will be the heat, but not necessarily in any exact proportionality, because the blanketing action of the planet's atmosphere is the principal factor in the retention of any heat which the surface may receive. Many geologists believe that a continually cloudy atmosphere, which would certainly reflect most of the sun's rays, but would retain terrestrial heat, was largely responsible for the growth of a tropically luxuriant vegetation within the Arctic circle in past ages. Morever, some highly important climatic properties, such as the melting of snow in high latitudes and the possibility or nonpossibility of a permanent ice-cap, depend on the maximum summer temperature, rather than on the mean temperature. The maximum temperature at the sub-solar point is somewhat intimately related to the reflection of *total* radiation spherically, and to some extent the latter follows the luminous albedo. The winter snows of Mars reach nearly as low a latitude as on earth, but no lower. The feebler sunshine of Mars is better conserved because it suffers a smaller reflective loss in passing through a clearer and rarer atmosphere. In fact, the albedo of Mars is so much smaller than the earth's that Mars would have the hotter climate of the two, in spite of greater distance from the sun, were it not that a rare atmosphere permits an easier escape of Martian surface radiation. On a planet with hardly any air, but having a long period of insolation and approximation to a steady state of thermal equilibrium, the sub-solar effect of the sun's rays must be nearly equal to the solar constant multiplied by one minus the spherical reflection of solar rays of every wavelength $(1 - A_1^{(t)})$. Thus, for the moon, the reflection of total radiation in connection with the temperature (both of which are measurable) has a bearing on the problem of the solar constant, although it may not be possible to utilize the information fully for lack of other data.

If a completely and uniformly diffusive reflecting sphere of indefinitely great radius be drawn about the sun as a center and including the earth, and if the central radius of the segment of this sphere in view from the night side of the earth be in the prolongation of the line from the sun to the earth, or what amounts to the same thing, if we imagine an ideal night sky to be "packed" with perfectly reflecting full moons, such a segment of the sphere (embraced in a hemisphere as viewed from the earth), or such a sky, should reproduce sunlight at the earth's distance. The moon sends us 1/98,317 part of the light reflected from such a hemisphere to a point,[1] and the full moon, which may be likened to a circular disk of 15′32.″7 radius cut out from this surface, should send us that fraction of sunlight, if it were not that it does not reflect in that way, but absorbs all but a small part of the combined luminous and nonluminous radiation received from the sun. Since, however, when equilibrium is attained, the combined emission and diffuse reflection of rays of every wave-length must equal the total of solar radiation received (unless there is some exceptional specular reflection in a particular direction, of which there is no evidence, and except for a slight retention of heat to be radiated away during the night) a measurement of the heat received from the total radiations of sun and moon, respectively, should approximate to this ratio. Such a measurement was made by Director Langley and myself at the Allegheny Observatory from which the fraction 1/96,509 was obtained, which seems to be in sufficiently close agreement with the theory.

Combining the above-named theoretical value with the observed ratio of $\frac{\text{sunlight}}{\text{moonlight}}$ at full moon, we have the following

Values of the geometrical lunar albedo:

By Zöllner's observation, $A_2 = \dfrac{98{,}317}{618{,}000} = 0.159,$

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[1] Namely, light from surface of hemisphere : light from lunar disk $= \dfrac{2}{\sin^2 \sigma} \times \dfrac{\sin^2 S}{\sin^2 s} = \dfrac{2}{\sin^2 15'\,32''.7} \times 1.00515 = 98{,}317 : 1.$

With Russell's adopted ratio, $A_2 = \frac{98{,}317}{465{,}000} = 0.211,$[1]

With Müller's adopted ratio, $A_2 = \frac{98{,}317}{569{,}500} = 0.173.$

Mean $A_2 = 0.181.$

Müller points out that numerical values will differ according to the definitions of albedo of which there are several. Of three theories given in his book with much detail, neither one is even remotely applicable to the moon, except in so far as the particular values by the Lommel-Seeliger and Euler theories *for full phase* do coincide with the geometrical albedo, on account of the aforesaid identity of the equations for this special case. The albedoes "by Seeliger's definition" which are set down by Müller are obtained on the limiting assumption that the coefficients of absorption of incoming and outgoing rays have the ratio $\lambda = 1$, which makes the coefficient in the Seeliger formula for $A_2 = 2$, or the same as in the formula for geometrical albedo. Otherwise, if λ differs from unity, we have

$\lambda = 1$, numerical coefficient = 2.0000
$\lambda = 2$, numerical coefficient = 1.8924
$\lambda = 3$, numerical coefficient = 1.8679
$\lambda = 4$, numerical coefficient = 1.8628
$\lambda = 5$, numerical coefficient = 1.8639
$\lambda = 6$, numerical coefficient = 1.8673
$\lambda = 10$, numerical coefficient = 1.8846

The albedoes "by Lambert's definition" are spherical albedoes derived from the geometrical albedoes by applying the factor $q = 0.75$, which is obtained by integration of the Lambert phase-curve. Müller leaves the reader to choose for himself between these values of the moon's albedo:

"$A_1 = 0.129$ (by Lambert's definition),
$A_2 = 0.172$ (by Seeliger's definition)."[2]

The use of the Lambert theory and of the constant factor 0.75 in passing from A_2 to A_1, prevents the values of A_1 in Müller's book from being regarded as spherical albedoes, except in those cases where Lambert's law may possibly be followed approximately.

[1] **Russell himself, as already noted, divides this by 2 to get his "p," obtaining $p = 0.105$, and for Zollner's value, $p = 0.08$.**

[2] ***Photometrie der Gestirne*, p. 343.**

With my value of the earth: moon ratio and Müller's geometrical albedo of the moon, the earth's geometrical albedo is

$$A_{e2} = 4.8 \times 0.172 = 0.826.$$

The ratio 4.8 : 1 applies only to the geometrical albedoes. The spherical albedoes adopted by Russell, namely,

$$``A" = A_{m1} = 0.073 \text{ for the moon,}$$

$$``A" = A_{e1} = 0.45 \text{ for the earth,}$$

have the larger ratio $A_{e1} : A_{m1} = 6.16 : 1$, and I shall show presently that this ratio ought to be still further increased. On the other hand Russell's values of p which are proportional to geometrical albedoes have a ratio smaller than mine, namely:

$$p^{(e)} : p^{(m)} = A_{e2} : A_{m2} = 3.86 : 1,$$

and one which does not agree with his adopted ratio of sunlight to moonlight. A revision on this account is certainly required.

Russell's lunar value, "$p = 0.105$," if it represents the "reflecting power" of the lunar surface,[1] would require that the moon should be composed of something almost as dark as dark grey slate, or nearly like trachyte lava, 0.10, according to the figures which he quotes from Wilsing and Scheiner. But excluding the very brightest and darkest spots which are of relatively small area, there are extensive dark regions on the moon whose average total-radiation reflection (bolometrically determined by measuring the transmission of lunar radiation through a glass plate which cuts off practically all of the emitted rays and distinguishes between these and the reflected ones) is from 10 to 12 per cent., while that of correspondingly situated bright regions (similarly determined) is from 20 to 25 per cent. Since the moon's surface is about equally divided between such "dark" and "bright" areas, a mean total-radiation reflection of 0.15 to 0.185 (average = 0.168) is indicated by my bolometric measures which form a useful check on my photometric results.[2]

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[1] Russell says (*op. cit.*, p. 192): "Wilsing and Scheiner have determined the reflecting power of many ordinary rocks, using an approximately flat, rough, natural surface normal to the incident and reflected rays. Their formula of reduction gives exactly the quantity which has been designated by p." To the writer, it looks as if p, the planetary illuminating power, should be multiplied by 3/2 before making this comparison.

[2] Some samples of these are to be found in my "Photometry of a Lunar Eclipse," *Astrophysical Journal*, November, 1895, p. 299-300.

If the sum total of reflected rays of every wave-length agrees approximately with unaltered solar radiation, the preceding fraction must be increased a little to represent the result as it would be found outside the earth's atmosphere; because the solar reflected rays are of shorter wave-length than the rays emitted by the moon, and they are differently modified in passing through the atmosphere, which alters the relative values of the terms of the comparison. Except for certain bands of selective absorption, the longer waves are more readily transmitted by the air. It becomes increasingly evident that the solar constant is about 3.5 (C. G. Min.), but this is reduced to 1.5 at sea-level, so that the real transmission of solar rays by the atmosphere is 3/7. In a seasonally comparable observation of the moon, 48 per cent. of its emitted radiation entered through the air. Reducing to conditions outside the atmosphere by these values,

$$\frac{\text{radiation reflected by the moon}}{\text{radiation absorbed by the moon}} = \frac{16.8 \times (7/3)}{83.2 \times (10/4.8)} = \frac{1}{4.42},$$

and the true percentage of total solar radiation reflected from the moon is 100/5.42 = 18.5%, which differs little from a mean of the three results quoted for the reflected light of the moon, $A_2 = 18.1\%$. These, however, as I shall show, need to be diminished somewhat.

The question whether the invisible and longer solar waves of radiation are better or worse reflected by the moon than the visible ones has never been definitely settled, and indeed there is diversity of opinion as to the relative reflection by the moon of different colors in the visible spectrum. We need not consider the great bands of "metallic" reflection by quartz near 9μ and the large reflection by many common terrestrial substances between 8 and 10μ, for there is very little solar radiation of these wave-lengths to suffer reflection.[1] Metals have greater specular reflection for infra-red radiation just beyond the visible spectrum than for luminous rays; but metals are not in question here. The lunar reflection is almost entirely diffusive, and we wish to know how substances which reflect diffusely behave to infra-red rays between 0.7 and 3.0μ. Eighteen years ago, I published the value of 13.1

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[1] See my paper on "The Temperature Assigned by Langley to the Moon," *Science*, N. S., Vol. XXXVII, No. 964, pp. 949-957, June 20, 1913.

per cent. for the lunar reflection of total solar radiation,[1] but I now think that this should be increased to the value given above, (18.5%), because in my former work I under-rated the absorption of solar radiation by the air.

On the other hand, on the strength of Zöllner's oft quoted, but little studied value of what he calls the "true" lunar albedo (17.4%) I had formerly supposed that luminous rays are better reflected than the visible ones from 0.7 to 3.0μ; but it now appears probable from measures which are to follow, that this relation must be reversed, and that the larger luminous reflection which would result from the lunar-solar ratios adopted by Müller and Russell, can not be accepted. In fact, in place of Zöllner's hitherto accepted albedo must be substituted the smaller value $A_2 = 0.159$, which follows from his own observations (entirely apart from any considerations whatsoever as to the shape of the moon, or as to its surface quality, or the peculiarities of its phase law).

I will now give a series of ratios of sunlight to moonlight for homogeneous radiations in the visible spectrum derived from my spectro-photometric observations, published in *Astronomische Nachrichten,* Nr. 4820 (s. 385—386) which, as there given, are corrected for atmospheric absorption only. The original values are all that is necessary for a comparison of the relative reflection of different colors by the moon, but for our present purpose they

λ	Sunlight/Moonlight	λ	Sunlight/Moonlight[2]
μ		μ	
0.40	531,000	0.56	682,000
0.42	559,000	0.58	666,000
0.44	587,000	0.60	646,000
0.46	607,000	0.62	619,000
0.48	640,000	0.64	600,000
0.50	669,000	0.66	513,000
0.52	681,000	0.68	456,000
0.54	685,000	Mean =	609,000

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[1] *Astrophysical Journal,* Vol. VIII, p. 275, December, 1898.

[2] The reduction factor to moon's mean distance from the earth, and to full moon, rests on the following data:

	Series 1	Series 2	Series 3
Mean phase-angle from full,	$a = -30°$	$a = -18°$	$a = -6°$
Light (from lunar phase-curve),	0.53	0.69	0.90
Moon's parallax,	3374″	3415″	3455″
Reduction factors,	0.515	0.687	0.916

require further reduction for the distance of the moon and for the interval to exact full moon. The original figures have been multiplied by the factor 0.70 and are fully corrected. The spectro-photometric method yields results which have one special advantage. They are entirely free from the troublesome Purkinje effect which has vitiated much of the previous measurement.

The mean ratio of sunlight to moonlight for light of every color within the visible spectrum is

$$\text{sunlight} : \text{moonlight} = 609{,}000 : 1,$$

but considering that the central region in the green affects the eye most powerfully, a mean visual ratio of 681,000 : 1 is to be preferred.

It is evident from inspection of the numbers in this table that, while the reflection of blue and violet light by the moon is larger than that in the green and yellow, there is also a large reflection in the red which increases in the direction of the infra-red. Unfortunately, there is a dearth of observations of the reflective power of ordinary terrestrial materials in the region between 0.7 and 3.0μ, where there is a great block of solar radiant energy; but I think we may conclude that this region is probably more reflected by the moon than the visible part of the spectrum. In this case, Russell's ratio (1 : 465,000) may answer well enough for the reflection of solar infra-red radiation by the moon, but it is much too large for the reflection of visible rays.

The earth can not have the same ratio of reflection for visible and infra-red rays that the moon does, because the earth's reflection is mainly atmospheric, with visible rays somewhat better reflected than the infra-red. If the moon's geometrical albedo for visible rays is $A_{m2} = 0.15$, that of the earth is $A_{e2} = 4.8 \times 0.15 = 0.72$; but the reflection of total radiation by the earth, unlike that by the moon, is smaller than for visible rays (because the infra-red rays are but little reflected by the atmosphere). It can hardly exceed $A_{e2}^{(t)} = 0.70$, and may be as low as 0.50. For the present I shall adopt $A_{e2}^{(t)} = 0.60$. *Whatever values are finally adopted ought to be consistent among themselves and with the general principles now under discussion.*

In my visual photometric work on the earth-shine, the intrinsic brightness of the moon at quadrature is taken = 0.16 times the light at full moon, giving full-moon light: full-earth light = 1,600 : 0.16 = 10,000 : 1, since I found that full earth-shine at the time of new moon must be about 1/1,600 of the light of a corresponding sunlit area of the moon near quadrature.[1] It was not necessary for this purpose that the whole illuminated surface of the moon should be measured, nor is it possible to make such a measure of the earth-shine directly at new moon; but it is essential that the lunar surfaces to be compared shall be similar, and Professor Russell's arbitrary change of my mean ratio is not admissible, even after granting the large probable error of the result.[2]

I propose to give equal weights to the results of my own measures and to those of Zöllner as now correctly reduced. Comparing the lunar-solar ratio with the moonlight: earth-shine ratio given above, we have (with Very's value)

sunlight : full-earth light = 681,000 : 10,000 = 68.1 : 1,

(with Zöllner's value)

sunlight : full-earth light = 618,000 : 10,000 = 61.8 : 1.

Allowing for the greater area of the earth as seen from the moon, these ratios become:

68.1/13.4 = 5.08, and 61.8/13.4 = 4.61, mean = 4.8.

Moon's geometrical albedo (for the visual effect)

$$\left.\begin{array}{l}\text{According to Very, } A_{m_2} = 98{,}317/681{,}000 = 0.144\\ \text{According to Zöllner, } A_{m_2} = 98{,}317/618{,}000 = 0.159\end{array}\right\}\text{mean} = \mathbf{0.15}$$

Earth's geometrical albedo $= 4.8 \times 0.15 =$ **0.72**

Geometrical reflection of total radiation:

Moon = **0.185**, Earth = **0.60** (?)

I take the reduction factor for spherical albedo, $q = 0.35$ for the moon from the integration of its phase-curve, and twice this, or $q = 0.70$ for the earth, which is a little less than the Lambert value.

Spherical albedo of moon, $A_{m_1} = 0.35 \times 0.15 =$ **0.053**,

Spherical albedo of earth, $A_{e_1} = 0.70 \times 0.72 =$ **0.504**.

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[1] *Astronomische Nachrichten*, Nr. 4696, s. 286.

[2] With the increased assurance given by the good agreement of the photographic result, I do not believe that this error can amount to as much as 10%. Some weighting of the observations is perhaps desirable.

Moon's Stellar Magnitude:

Difference of magnitude from sun = log 681,000/0.4 = + 14.58
Stellar magnitude of sun (Russell) = — 26.72
Stellar magnitude of moon (Very) = — 12.14
Photographic magnitude of moon (King) = — 11.37
(with Russell's phase-curve, etc.)
Moon's Color-Index (King-Very) = + 0.77,

or a little less than that of the sun,[1] which agrees with Abney's photographic observations, confirmed by my spectro-photometric comparison of sun and moon, in showing that the moonlight is bluer than sunlight. I have shown that the moonlight is redder than sunlight in the extreme red, but these rays do not count for much either photographically or visually, and therefore do not effect the color-index as usually defined.

Earth's Stellar Magnitude (Very):

As seen from moon, — 12.14 — 4.58 = — 16.72
As seen from sun, — 16.72 + 12.95 = — 3.77

I make no further claim for my previously published value of the earth's albedo[2] than that its ratio to the moon's albedo has been fairly well determined. The previous figures were based upon Zöllner's published lunar albedo and must be diminished a little according to what precedes; but this will not effect the argument for a high value of the solar constant, because this rests on wholly different grounds. If I could accept in principle Professor Russell's argument that my measurement of the earth-shine, "far from being inconsistent with Abbot's value of the solar constant (1.93 calories) is actually in agreement with it,"[3] since it has now been shown that Russell's p must be doubled to give the geometrical albedo, I might claim that Abbot's constant should be doubled! But unhappily this simple method of disposing of the solar-constant problem will not work. A high value of terrestrial reflection of total solar radiation is indeed inconsistent with a low value of the solar-constant, but a low value of this reflection is not necessarily inconsistent with a high value of solar radiation, because the atmospheric depletion of the sun's rays is composed of several parts. If reflection is found to be less potent than has been supposed, this simply puts a heavier burden on the other processes of depletion.

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[1] Color-index of sun = between + 0.8 and + 0.9, that of Capella being + 1.0.

[2] *Astronomische Nachrichten*, Nr. 4820, s. 400.

[3] *Astrophysical Journal*, April, 1916, p. 195.

I have elsewhere concluded[1] that only about 18 per cent. of the sun's rays, received upon the entire sunward hemisphere of the earth, are effective at the earth's surface in production of temperature. Out of the 82 per cent. of solar radiation lost by the sunward hemisphere of the earth in one way or another in passing through the earth's atmosphere, the measurement of the earth's spherical albedo which has just been given indicates that approximately 50 are reflected back to space by air, or by clouds, including a reflection of a few per cent. by the solid or liquid surface of the earth. The rest of the depletion is divided among agencies which go under the general name of "absorption," but this also is really a complex of several processes.

As was pointed out in my "Note on Atmospheric Radiation,"[2] a portion of the incoming solar radiation of short wave-length is used up in the upper air in ionization of atmospheric ingredients, or in the production of ozone and other highly efficient absorbents; and since there is at present no way of finding out how potent this part of the atmospheric process may be (except possibly through an interpretation of certain little understood facts made known to us in the study of atmospheric thermodynamics) it is possible, as Dr. Louis Bell has suggested to me, that more solar energy than we imagine is lost in the ionization processes; and in this case quite a little of the remaining 32 per cent. of "absorption," so-called, may be ionization by solar radiation of very short wave-length at great altitudes in the atmosphere, these rays being wholly obliterated in the process.

I have alluded to the changes which Professor Russell has introduced into my earth-shine measures as founded on misapprehensions, and must now substantiate this claim. On page 185 of his second article we are told that "Table IVA contains data derived from Very's paper"; but the mode of derivation is not consistent. The first three numbers in the last column have been obtained by *multiplying* the ratio of exposure-durations for earth-shine and for sunlit moon by the ratio of photographic intensities;

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[1] *Astrophysical Journal,* Vol. XXXIV, p. 382, Dec., 1911.

[2] *American Journal of Science,* Vol. XXXIV, p. 533, Dec., 1912.

but the last two numbers have been found by *dividing* the first quantities by the second. By this means, the two sets of numbers (for January and August respectively) which, if they had been correctly derived, would have been entirely different, because as yet uncorrected for photographic peculiarities, are brought into seeming approximate agreement, and the conclusion is reached that the photographic correction which I have derived for the relatively over-exposed lunar spectrograms was unnecessary, and that my corrected values are wrong! By this wholly erroneous argument, Russell supports his reduction of my ratio of the earth's albedo to the moon's from the spectrograms, to a quantity about half as great as mine. It is needless to say that the seeming agreement of the numbers in the last column of Table IVA is wholly accidental.

On page 184 (*op. cit.*) Professor Russell says that my "conclusions regarding the relative intensity of the light of these two sources [the earth-lit and the sun-lit portions of the moon] depend on assumptions regarding the photographic action of exposures to light of different brightness." On the contrary, my results do not rest on "assumptions," but on carefully executed quantitative measurements of the photographic effects in question throughout the entire visible spectrum. When properly reduced, there is no difference between the results of the photographic and of the visual observations. The statement (*op. cit.*, p. 186) that "the photographic observations therefore make the earth-shine only half as bright as do the visual observations," is consequently entirely wrong; and the conclusion that "this is just what might be expected if the plates had followed the ordinary law for faint illumination and long exposure, and been 'less sensitive than $i \times t$'," is equally erroneous. The error has come from the incorrect reduction of my observations by Russell in the aforesaid Table IVA.

The discrepancy which Professor Russell thinks he finds between my theory and my observations in connection with the phase-curve of the moon (*op. cit.*, p. 186 to 187) does not really exist. The observations had first to be reduced to a constant unit of comparison surface, and then to a selected lunar phase-angle. As it happened, the two corrections in a particular case were of equal numerical value, but opposite sign. Limited areas of the moon were necessarily observed, and the comparison in every case in the visual measures was between "twin circular apertures in black card, each subtending 7′ of arc on the celestial sphere, one above, and the other below the horizontal line of junction of the last pair of reflecting prisms."[1] One of these apertures was illuminated by the standard light, and the other by a sample of the lunar surface. In preparing the material of *A.N.* 4696 for the press, I have omitted a remark which is needed to complete the sense. After the first equation at the top of page 286 in that paper, there should be inserted these words: "The ratio of intrinsic brightness for the particular (limited) region of the moon under observation is the inverse of that just given." A quotation from my note book will clear up the matter fully: "At $\varphi = 44°$, $M = 0.090$. At $\varphi = 87°$, $M = 0.058$. Ratio $= 1.55 : 1.00$. For equal moonlight E/M (for $\varphi = 44°$) must be multiplied by 1.55." The phase-reduction required *division* by the same number. This peculiarity is due to an exceptionally large reflection from the lunar substance when the angle of incidence is large and nearly equal to the angle of reflection, as was the case for the particular lunar region observed with the moon's elongation, $\varphi=44°$; but the reflection diminished as the angle of incidence of the solar rays on this spot decreased. The Lommel-Seeliger law was formulated to deal with just such peculiarities. My published result:

$$\text{“}E_{44} : E_{87} = (0.0003477 \times 1.55) : 0.0002210$$
$$= 0.0005389 : 0.0002210$$
$$= 2.438 : 1,\text{”}$$

(*op. cit.*, p. 286) still stands and is fairly comparable with the ratio for Venus, computed by me from Müller's result, namely,

$$V_{44} : V_{87} = 2.100 : 1,$$

or as Russell gives it for elongations 30° and 90°,

$$V_{30} : V_{90} = 2.74 : 1.00,$$

where Lambert's law would give 2.94 : 1.00. The agreement is close enough to show that the phase-law for the earth resembles that for Venus, approaching, however, a little more nearly to the Lambert law, and is quite different from that for the moon.

On page 189, Professor Russell says: "Very's observations of the earth-shine indicate that the mean full earth, as seen from the moon is forty times brighter than the full moon as seen from the earth," where the forty should be *sixty-eight.*

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[1] *Astronomische Nachrichten*, Nr. 4696, s. 269.

Russell's adopted value of the moon's stellar magnitude (— 12.55) is 0.41 magnitude brighter than mine (— 12.14). Zöllner's result from a comparison with the sun gave the intermediate value,— 12.24, while that from his Capella comparison, —12.18, approaches still more nearly to mine.[1]

The last line of Russell's Table V. (*op. cit.*, p. 190) which purports to give "the earth (from Very's reductions of Slipher's spectrograms)" is misleading, since he has substituted his own reduction for mine.

A final word may be permitted on the vicissitudes of the solar-lunar light-ratio. Bouguer, who obtained a ratio of 300,000 : 1 for sunlight to moonlight (*Traité d'Optique*, p. 87, 1760) was careful to observe when the full moon was near its mean distance and when both bodies were at the same altitude; but unfortunately, he thought it necessary to use identical optical means in either case, and therefore his candle had to be at a distance of 50 feet for the moon and 1⅓ feet for the sun, so that the illuminations actually measured were in the ratio of 1407 : 1, both moonlight and sunlight having been much reduced. Under these circumstances, the bluer light of the heavenly bodies being compared with reddish candle light, the moonlight, on account of its greater faintness and of the relatively greater sensitiveness of rod-vision for faint blue light as the general illumination diminished, had an undue advantage, in the candle comparison, over sunlight, as will be evident from a short table in my paper on "The Earth's Albedo."[2] Bouguer's 300,000 must be at least doubled to correct for this error. The spectrophotometric method entirely removes this difficulty.

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[1] **From Zöllner (*op. cit.*, p. 125 and p. 105).**

Log ratio Sun : Capella = 10.7463
Log ratio Sun : Moon = 5.7910

Log ratio Moon : Capella = 4.9553
Log ratio divided by 0.4 = — 12.39
Stellar magnitude of Capella = + 0.21

Stellar magnitude of Moon = — 12.18

[2] ***Astronomische Nachrichten*, Nr. 4696, s. 276.**

Dr. W. H. Wollaston, who found[1] that the sun =

$$5{,}563 \times (12)^2 = 801{,}072 \text{ moons},$$

used better observing conditions, but he made only two readings on the moon. In one observation at full, his candle was placed at 12 feet. In the other, made at a time when, if the atmosphere had been equally transparent the light should have been 0.84 of that at full moon, the same candle-reading "12 feet," was recorded. One can not help surmising that neither reading was better than a rough approximation.

Bond's solar-lunar ratio, 471,000 : 1, is an underestimate for the same reason that Bouguer's value is too small. Zöllner's criticism of Bond's fireworks as not accurate enough for standards is also fully justified.

Zöllner's own measurements of the ratio of sunlight to moonlight appear to have been made with great care; but in reducing them he becomes lost in the mazes of an unnecessarily complex argument. With the removal of this blemish, no fault can be found with the new value deduced from the original measures. The same can not be said of Zöllner's isolated measurements of the earth-shine[2] which require unknown corrections for skylight. The earth-shine observations of Arago and Laugier have been utilized by me in conjunction with my own with which they are in good agreement.

Various other more or less aberrant values of the moon's albedo usually err from inadequate correction for changes in atmospheric transparency. As an instance of a great name attached to an extraordinarily small value which is simply impossible, may be cited that of William Thomson (Lord Kelvin): "70,000 : 1" for the ratio of sunlight to full-moon light.[3]

Whatever faults may still remain in the values which are given here, they at least have this merit, that they are consistent among themselves, which is very far from being the case with the results which have been published hitherto.

WESTWOOD ASTROPHYSICAL OBSERVATORY,
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[1] *Philosophical Transactions* of the *Royal Society of London,* Vol. CXIX, p. 19-27, 1829.

[2] *Poggendorff's Jubelband,* p. 624, 1874.

[3] *Nature,* Vol. XXVII, p. 279, January 18, 1883.

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# THE LUMINIFEROUS ETHER:
(I) ITS RELATION TO THE ELECTRON AND TO A UNIVERSAL INTERSTELLAR MEDIUM;
(II) ITS RELATION TO THE ATOM

BY
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LUNAR AND TERRESTRIAL ALBEDOES

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HISTORICAL NOTE

This paper was presented in abstract at the twenty-second meeting of the American Astronomical Society at Harvard College Observatory, August 20, 1918, and is here reprinted in full from the *New-Church Review* for October, 1918 (Vol. XXV, pages 528 to 576). It is an elaboration of a view put forth in my critique and appreciation of Swedenborg's *Principia* which appeared in the same publication in April and July, 1913 (Vol. XX, pages 161 to 197, and 392 to 436), where a return to Swedenborg's scientific doctrine of a limited ether, and of light as consisting of discrete vibrating ether-particles, was advocated. The present paper includes much additional material, especially in respect to the connection of the ether with a fundamental electronic unit and the universal medium out of which both the electron and the ether-particle are supposed to be formed. Incidentally, further details are included from my papers on the cause and limitation of gravitation which have not, as yet, been published elsewhere.

THE LUMINIFEROUS ETHER: (I) ITS RELATION TO THE ELECTRON AND TO A UNIVERSAL INTERSTELLAR MEDIUM.

THERE has arisen at the present time a new doctrine which declares that there is no ether, or universal atmosphere consecrated to the propagation of luminous waves, but that light consists of discrete "quanta" of luminous energy, thrown off from vibrating electrons and originating in their disturbed motion. This proposition appears to be well founded; but it does not therefore do away with the necessity for a universal medium, though this medium should no longer be called "the luminiferous ether." I have adopted for the universal interstellar atmosphere the name of "aura," a term which I have borrowed from Swedenborg, who uses it sometimes to designate a universal atmosphere, a "purer ether," assigning to it in the end a gravitational function, and placing it as an antecedent to the light-bearing ether.

While the universal aura, or "ether of space," as Sir Oliver Lodge calls it, has no mass, the case is different with the electrically organized "ether," of which Sir J. J. Thomson in his "Electricity and Matter" has said: "All mass is mass of the ether, all momentum, momentum of the ether, and all kinetic energy, kinetic energy of the ether." Here, however, he points out that "the concentration of the lines of force on the small negative bodies—the 'corpuscles' [or electrons]—is so great that practically the whole of the bound ether is localized around these bodies"; and this follows because the energy per unit volume in the magnetic field of the moving electrons diminishes in proportion to the inverse fourth power of the distance.* This alone shows that gravitation can not reside in the electrically bound ether, since the efficacy of gravitative force is related to the inverse *square* of the distance; nor can gravitation depend upon the mere revolutions of the electrons, though these, if uncompensated, may generate an atomic *magnetic* field. On this account and because the magnitudes of the electric and gravitational forces are of a totally different order, we are driven to seek for some mechanism for gravitation different from the disturbance produced by a simple electronic revolution *per se*.

It is assumed by Thomson that his corpuscles are spheres. It does not appear possible that electrons can have the ring-vortex form which Lord Kelvin assigned to his vortex-atoms; for these could not unite into vortex filaments, or unite in any way except by two neighbors alternately threading each the other periodically.

*William Sutherland has considered the reason for this law in an article on "The Electric Origin of Molecular Attraction" (*Philosophical Magazine,* Ser. 6, Vol. IV, p. 625, December, 1902). "The theory of magnetism familiarizes us with an inverse fourth-power force between magnets at distances great compared with their lengths. In applying this known magnetic result to account for molecular attraction, we are at the outset confronted with the difficulty that in the case of magnets the force is as often repulsive as attractive, the nature of the force depending on the relative direction of polarities in the magnets, whereas the molecular forces required to account for cohesion must be preponderatingly attractive." On further examination, however, he finds that "the attractive forces by their own operation tend to increase themselves, while the repulsive forces tend to decrease themselves." Thus if neighboring magnets, or electric doublets approach each other, the "axes tend to take the same direction, and therefore on this account they exercise a stronger attraction on one another; and also because attracting forces varying inversely as the fourth power of the distance produce motion which increases their strength, there are two causes which make the attractive forces amongst a number of moving doublets of more dynamical importance than the repulsive." The applicability of the law to any medium rests upon the magnetic properties of its least constituents.

Swedenborg's "ethereal particle" was also supposed to be spherical, and was thus fitted to receive all kinds of vibration without distinction. "The ethereal particles thus formed can subsist under any form of motion and with perfect aptitude to it" (*Principia,* Part III, Chap. V, No. 6.) But more noteworthy was his conception of a peculiar sort of vortex-particle, to which we shall return, having a structure of such a nature that similarly rotating particles may unite into vortex filaments. Such a structure lends itself admirably to the explanation of several phenomena, such as the polarization of the magnetic medium under the influence of an electric circuit, and leads especially to an idea of the way in which ether-particles are formed by electrons out of the aura through the aid of forces in this medium; and as if foreseeing such a relation, though there were then few facts on which to base it, Swedenborg assigned an ethereal origin to electricity as well as light, and had a vague idea of a distinction between "warm and cold light," or, as we should now say, between the radiation of a heated body and electrically produced luminescence, linking the "cold light" with electricity. He also assigned both a vibratory and a corpuscular nature to his luminous "ethereal particle," thus combining the Newtonian and Huyghenian hypotheses—a view to which we are in a fair way to return.* After several speculations of this sort, he restrains himself with the words: "Much more might be said, but which I omit, as I have no experiments to prove my statements."

The vortical form in question was assigned to a particle of the magnetic medium. It had a rational basis in the fact of the obvious existence of vortex motion in that medium, as revealed in the magnetic phantom; and some of its properties have seemed to be too valuable to be laid on the shelf of unverified hypothesis. There is an opportunity for a resuscitation and modification of this scheme today. From the same mine of suggestions, now happily capable of a certain amount of confirmation by experiment, I think we may borrow, with some modifications, a working plan of an atom.

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*For a fuller account of this subject, see my "Prevision of Scientific Progress." Boston: B. A. Whittemore, 1914.

Let us assume that there is around an electron a condensation of the aura in which its electric properties reside; that the electron both rotates on an axis and also pulsates with a radial, regular, and uniform motion at all points of its surface, being urged to this motion by an internal resilience; and that its shape is that of an approximate sphere with a central core, for which a reason will be given later. The electron is incapable of further compression, but is surrounded by its own ethereal "atmosphere" in which the pressure diminishes outwardly at a very rapid rate. During steady motion, this atmosphere moves with the electron and virtually forms a part of it; but the adhesion is so slight that under sudden shock the envelope is stript off and becomes a spherical ethereal particle which pulsates and oscillates within limits, but does not rotate. The naked electron immediately takes to itself a new atmosphere formed out of the illimitable aura, and may almost immediately lose its garment again at the next shock. Thus a single electron can undergo a succession of shocks which generate a series of ether-corpuscles, each with a different, but harmonically related variety of vibration, which could not happen if the electron as a whole were immediately converted into an ether-corpuscle.

Professor W. H. Bragg, who was the first to indicate the probable corpuscular nature of the Röntgen rays, suggested that "an electron of given energy may be *converted* into a light-quantum of equal energy and *vice versa.*" This at first seemed a permissible interpretation of the experimental evidence; but now that it is known that *the X-rays carry with them the peculiar vibrations of the electrons characteristic of the atoms composing the anti-cathode,* it becomes evident that this is not an immediate conversion of electrons in the cathode stream into ether, but that the electrons from the cathode are absorbed by the target and in this process they produce disturbances in the electrons and the ether already attached to its atoms. The electron therefore preserves its identity, and is *associated* with the ether, rather than converted into it.

The ether-particle thus formed is a sphere of the same size as the electron. Its pulsation gives it mass which, together with its onward motion, causes it to have momentum. Its oscillation gives it alternating positive and negative electric sign and a reciprocating magnetic field, the whole constituting a least quantum of electro-magnetic energy. Because the electrons everywhere repel each other, and because radiation is an affair of surfaces, the emission of radiant ether by any portion of matter, from its origin, takes the form of a spherical wave.

Conjoined with its electron, the ether-particle may be said to have had a previous existence of a relatively quiescent sort, but as a free entity it necessarily moves onward with the velocity of light, carrying with it its own invariable quantum of energy. Consequently, the energy of radiation is measured by the number of ether-particles in the unit of volume. This follows from Planck's epochal discovery that the radiant quantum is a fundamental constant of nature. Eventually, the ether-particle, which has been formed out of the aura, is re-absorbed by the medium; but because the frictional resistance of the aura is very minute, the progress of absorption is excessively slow.

The Assumption that the Ether-Particle is derived from an Electronic Envelope.—A fundamental difference between a free ether-particle and an electron is that the latter rotates on an axis, and has polarity, but the former does not rotate and, having a perfectly spherical surface, is thereby free to take up any form of oscillation and transmit it with complete identity as to all of its phases.

The electrons which issue from the cathode of an X-ray tube, strike the atoms of the target; whereupon certain electrons attached to these atoms have the rotations of their envelopes transformed into a rocking motion, and the stripped-off envelopes proceed as vibrating ether-corpuscles; for the X-rays, no less than the luminous rays, are corpuscular "quanta" of radiant energy, characterized by an extraordinary rapidity of vibration, which, since it originates in an electronic oscillation, I conceive to be an *oscillatory* vibration.

An advancing electron, moving in the direction of its axis, produces by its rotation and carries along with it a vortex-filament of the aura which is the electron's magnetic field, due to its motion and made up of circular "lines of magnetic force", or currents of minute size in the adjoining aura, concentric with the axis of the electron. Sir J. J. Thomson supposes that the electron is also accompanied by radial vortex-filaments which are its *electric* lines of force; and that when the velocity in the axial direction increases, these lines crowd towards the equatorial plane thus giving greater resistance to motion, or greater "inertia." According to my present proposition, the steady orbital motion of the electrons within the atom is *always* substantially in the direction of their axes of rotation, but at speeds much less than that of light.

If the lines of electric force about an electron are vortex-filaments, this would seem to imply that the magnetic aura is itself composed of particles still smaller than the electrons, which are also rotating and which become polarized, or arranged in radial filaments under the strain in the medium set up by the rotation of the electron, perhaps combined with some other form of motion. This suggests that electrical attractions and repulsions may be associated with the rotations of the electrons, attended by vortices in the aura, but that gravity originates from waves of longitudinal vibration in the aura (the counterpart of sound waves in the air) produced by the rapidly alternating expansion and contraction of the electrons. This explanation requires that there must be a perpetual elastic action and reaction in alternation between the inner contents of the spherical electrons and the volume of aura which fills an "aural cell," at an electronic surface of transition which is not far from spherical and of fixed dimensions determined by some property of the aura yet to be investigated.

On the supposition that an electron is a rotating sphere, or near sphere, of aura when translated at moderate speed, it does not follow that the shape will be retained at higher speeds. More probably any accentuation of the progressive motion will be accompanied by increased resistance with transition from a sphere to an oblate spheroid, and final dissipation by centrifugal force if the speed were to exceed the velocity of light, for which the resistance would be infinite with the lines of electric force compressed into an infinitely thin sheet. But a spherical particle of ether which is without any rotation, moves at once with the velocity of light by virtue of the properties of the aura out of which the ether is made. I shall show further on that the electronic rotation is so rapid that the shape is probably not merely spheroidal, but is that of an annulus or cored vortex-ring.

The electric field of an electron is therefore made up of static radial lines of strain in the aura, which are accompanied by a magnetic field of kinetic lines of flow whenever the electron advances, both together constituting a consistent electro-magnetic dynamic system in the aura.

Just as the larger Faraday tubes of electric force between oppositely electrified bodies are composed of stretched chains of polarized *atoms,* where the individual members of the chain are kept in connection by streams of moving electrons, so the finer threads of electric force about the electrons, repeating the structure on a smaller scale, require chains of still finer polarized particles of the aura,—the entire electric system comprising three orders of magnitude, namely, atoms, electrons, magnetons. Here, however, by an alternation of properties which seems to be inevitable in the relation and connection of these different orders of magnitude, the connecting lines of the aura are not lines of flow, but are made up of rotating magnetons held together by magnetic attraction; for all the properties of the aura, as a whole and as to its least units, are magnetic. The magnetons lend themselves as naturally to *rotations* as the electrons do to the *flow* of electric current.

Thus the normal motion of an unrestrained electron is a rectilinear electric flow, carrying with it always an accompanying orbital circulation of magnetons. The latter is the magnetic field due to the progressive motion of the electron, while any magnetically originated flux is, first of all, either a circulation or a rotation of magnetons which constrains the electrons to flow according to their own peculiar mode.

According to Sir J. J. Thomson's view, X-rays, and therefore we may now say all luminous rays, since it has become obvious that there is no distinction of form between the two, are propagated rectilineally as kinks in a universally present system of ether tubes. But a discrepancy immediately appears, because, as we have seen, the ethereal structure "is localized," according to the same authority, and is by no means universal.

But Bragg has shown that the X-rays are probably of a corpuscular nature equally with the cathode rays, and since the electrons disappear in producing X-rays, while the latter, in turn, are capable of "generating," or more probably of setting free electrons anew, since the light-rays, when they meet an electronic system which can execute the same rhythm, or dance to the same tune, are absorbed, and the energy of the light is communicated to the electronic system, it seems fairly demonstrated that electrons and ether-corpuscles are structurally connected; and that they are identical as to magnitude is very probable, the chief difference between them being the presence or absence of axial rotation.

In regard to Thomson's fibrous ether, Professor Millikan, in his book on "The Electron" (p. 230), raises the objection that "When we maintain the field constant and vary the charge on the drop [of liquid to which an electron is attached, where the motion of the suspended drop is controlled by the electric field], the granular structure of electricity is proved by the discontinuous changes in the velocity, but when we maintain the charge constant and vary the field, the lack of discontinuous change in the velocity disproves the contention of a fibrous structure in the field, unless the assumption be made that there are an enormous number of ether strings ending in one electron." This seems to him improbable, but perhaps the ethereal envelope, attached to an electron, has an analogous structure formed out of the aura. If the aura is composed of polar particles which are minute in respect to the size of an electron, the radial filaments of polarized magnetons, attached to an electron and which constitute its electric field, are very numerous, fulfilling Professor Millikan's proviso in this respect; and if the convergence of these vortex-filaments is what produces the surface condensation to balance the relatively enormous centrifugal force of the whirling electron, they act like so many compressed springs.

The shaking off of the electronic envelope gives to the ether-particle thus formed an oscillatory springiness and provides it with an analogous system of vortex filaments by which it takes hold of the aura in transit, or else has its energy transferred to a new electron upon absorption. A considerable amount of energy becomes latent in the generation of an ether-corpuscle and is not included in the "radiant energy" which it conveys. This energy of formation is derived from the constitutional energy of the electron, and is proportional to the frequency of the luminous vibration, but *not to the radiant intensity*. This, it seems to me, is the meaning of Einstein's equation:

$$h/e = \Delta V/\Delta\nu$$

where h is Planck's energy-constant, e is the charge on an electron, ΔV is the change of the electrical potential, and $\Delta\nu$ is the corresponding range of frequency, the higher frequency requiring a correspondingly greater electric potential for its production. Lenard's discovery (*Annalen der Physik*, (4), Bd. VIII, S. 149, 1902) that in the photoelectric effect, the energy with which electrons are thrown off from a metal on exposure to ultra-violet light is wholly independent of the intensity of the light, requires that there must be an independent source of energy which is presumably structural. This structural energy of the ether was originally derived from the electrons, and may be returned to them.

The ether-corpuscle is a distinct entity, but it has a structure which has extension into a surrounding field of potential energy; and once organized, this structure can experience vicissitudes which are the "radiant energy" of the combination.

Distinction between Radiation from Satellite Electrons and from those of the Nucleus.—For a uniform revolution of electrons in an orbit, with a constant radius vector, there is no acceleration and no production of electro-magnetic radiation. The latter arises through perturbations which change the radius vector of the orbit, just as a uniform electric current in a conducting wire produces no induced current, but the latter arises when the first current is made or broken, thus as a result of a sudden *change* of current. Similarly, the removal of a satellite electron takes place by a series of jumps from one stable orbit to another, each explosive dislocation of the uniform orbital motion and substitution of a new orbit producing a radiant vibration of a special wave-length; and a succession of such dislocations causes a series of discrete, but harmonically related vibrations appertaining to a set of associated orbital distances and constituting a spectral series.

On the other hand, thermal shocks arising in the interrelations of the molecules in a solid or liquid, or in a highly condensed gas, and which are powerful enough to affect the entire spherical shell of electrons of the nucleus of an atom, produce continuous changes in adjoining electronic orbits distributed in parallel over the surface of the spherical shell. Thus if the nucleus of the atom expands, all of the nuclear orbits enlarge. The combination of expanding orbits is like an ever varying spiral motion with a continually changing radius vector. Whatever shocks these electrons of the general atomic sphere receive will generate a whole range of gradually varying frequencies giving rise to a continuous spectrum. Hence the continuous spectrum of a radiating solid must be attributed wholly to the vibrating nuclei of its atoms, consisting each of thousands or tens of thousands of electrons. As a rule (with a few partial exceptions as in the case of erbium, etc.) there are no linear intensifications in such spectra, that is, the satellite electrons play such a subordinate part in the transformation that their periods are not in evidence. The nuclear perturbations are a function of the body's thermal state. On the contrary, electrically produced *luminescence* arises from disturbances which affect the satellite electrons, and which do not necessarily touch the electrons of the nuclear sphere. The bright-line luminescence spectrum may proceed from comparatively cool material. Somewhat similar to this is the radiant emission of the firefly, where considerable radiation of short wave-length in the form of a broad spectral band, which if it were thermally produced would correspond to a temperature of several thousand degrees, is actually given off at the temperature of the living body without the corresponding infra-red rays of much greater total power which are ordinarily associated with low-temperature emission. This was determined by direct spectro-bolometric observations of the entire energy-spectrum from the visible rays through the infra-red.*

The Dual Aspect of the Rotating Electrons becomes the Ultimate Origin of the "Two Electricities."—The proposed hypothesis of the internal structure of an atom assumes that the atom consists solely of electrons revolving in orbits, that there is but one sort of electron, though it has two aspects, because it is a rotating spherical mass, or what is more likely, a cored vortex, or near-sphere, of the universal medium, and has a definite volume and rotational energy determined by the properties of this medium. There is but one *known* sort of electron, and if the foregoing definition be accepted there can be no other. The

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*See Langley and Very, "The Cheapest Form of Light," *American Journal of Science*, Vol. XL, p. 97, August, 1890.

hypothetical "positive electron" has never been isolated simply because it does not exist. The conception needs but a few minor details to make it perfectly definite and simple. It remains only to trace the consequences of these assumptions, and here there is room for much variety in the compounding of motions. Whatever atomic scheme is proposed, however, must conform in its results and properties to certain well established physical and chemical facts. The chemical and spectroscopic facts are already well on the road towards explanation through the hypotheses in respect to the orbits of satellite electrons which are accepted today. Naught but the nucleus need be considered here.

An aggregation of electrons which all have the same electric properties will evidently tend to expand by the mutual repulsion of the several entities, and will be dispersed unless controlled by some form of consentaneous motion which shall overcome this dispersive tendency. Given some mode of limiting the dispersal, the mutual repulsion of the parts will nevertheless tend to distribute them into a spherical shell. Hence an atom composed of electrons probably has the form of a hollow sphere, and must necessarily possess an even number of layers arranged in pairs with opposite electrification.

The electron itself has an analogous structure, since it is a spherical vortex by close approximation, as we shall see, formed from the aura by rotation and condensed at the surface where it sustains the whole pressure of the aura by the centrifugal force of this rotation, but rarefied within.

The interior of the atom is filled with aura, that is, the entire atom is an electronic and ethereal structure finited by motion out of the aura; and the aura itself (except in the immediate vicinity of the electrons) passes freely through the interstices of the structure and serves to nourish it, to animate it with energy (for the aura is a great reservoir of energy), and to govern and define the modes and properties of the structure.

In 1903 Eichenwald repeated Rowland's experiment with modifications, by causing an electrified glass disk, coated on both faces with metal, to revolve. If the dielectric carries the magnetic aura along with it, there should be compensation between the dielectric and metallic influences upon the aura, and an absence of magnetic effect. But the magnetic effect was found, which proves that the aura is immobile in respect to the motion of matter through it. The sun and planets are not floating in the aura which immediately surrounds them, like corks in a stream of water. If the universal magnetic medium flows through matter, or is not set in motion by the motion of matter in material progressions, the medium may nevertheless suffer a slight retardation in the vicinity of the matter. This will have the effect of a condensation of the aura. It is not necessary that the general motion of the aura shall coincide with that of the matter which is contained within the same volume of space; for the matter takes hold of an immense volume of aura by its gravitational lines of force and is thus connected with and controlled by the general aural movement, but is independent of the aural movement in the immediate vicinity. Thus the aura moves in its own vast galactic sweep and exercises a large-scale, or general control over the movements of matter, but is independent, or as if immobile to its lesser motions.

The "Saturnian Atom" and the "Moseley Number."—The spherical surface of the atom may be deformed into an oblate spheroid by a sufficiently rapid translation in the direction of the atomic axis, but any marked distortion from a symmetrical form would lead to a breaking up of the atom. Such distortion, however, becomes difficult if the electrons are in exceedingly rapid orbital revolution. It is supposed by Bohr that certain electrons occupy a circumferential position, arranged in rings about the nucleus in a common plane so as to have some analogy with the rings of Saturn, whence the arrangement has been called the "Saturnian atom," that they are thus revolving in orbits around a central nucleus, and that the frequencies of line spectra are determined by perturbations of these satellite revolutions. It is also known that the atom contains an enormous amount of energy which is invariable except in radio-active disruption of the atom. I assume that this inner invariable energy of atomic constitution is that of still other orbital electronic revolutions of great velocity within a central nucleus in which resides by far the larger part of the mass, and whose stability is far greater than that of the satellite orbits. The latter are relatively few, and we may assume that their number is that of the Moseley number, characteristic of each element, which varies between 1 for hydrogen and 92 for uranium. For the moment we may leave in abeyance the question whether the orbits of the characteristic electrons which give the Röntgen rays of the K series are within or outside of the nucleus. For illustration assume them to be inside. Presumably in this case the satellite system of hexavalent uranium is composed of six concentric rings of electrons, composed successively from the inmost to the outmost ring of

$$6, 11, 15, 18, 20, 22 = 92 \text{ satellites.}$$

This gives the number six to the inner ring in agreement with the valency.

The nuclear electrons, on the other hand, reach into the thousands (uranium, about 439,000), and the mass of the atom is practically that of its nucleus. This implies that the electron is the fundamental gravitational unit, and that its power of gravitational attraction is independent of its position in the atom, though the resultant inertia is connected with the gyrostatic properties of the revolving and rotating electrons.

Origin of the Gravitational Impulse.—The remaining problem is to find a mode of electronic interaction, diverse from the electrical one, which is capable of producing a gravitational impulse, and one which shall not only not endanger, but which shall conduce to the stability of the atom. Stability may conceivably be maintained electrically, if this were all, by the juxtaposition of two concentric spherical shells of electrons revolving in parallel circular strands in the same direction and with the poles of the electrons juxtaposed in the direction of the orbital motion as aforesaid (since it is obvious that the planes of rotation of the electrons must set themselves at right angles to the directing attractive forces) forming as many superficial circular vortex-filaments by their combined motions of revolution and rotation; while the electronic rotations are opposite on the inner and outer shells, giving to the members of a pair of shells opposite electricities. Since the inner shell is slightly the smaller of the two, we may suppose that it has fewer electrons and that the satellite electrons which agree in electric charge, or direction of axial rotation, with those of the inner shell of the pair, are left over and are needed to make up a number equal to the sum of the electrons in the outer shell, thus providing for the precise equalization of electric charges in the neutral atom.

It has been held by several theorists that gravity is of the nature of a longitudinal wave of compression in a universal medium. The simplest way to account for such a wave is to suppose that the electrons are pulsating rapidly, that is, that they alternately expand and contract equably on all sides. I can not see that this will interfere in any way with the simultaneous movements on which the electric and magnetic properties of the atom depend, while the need of a wholly independent origin for gravity seems obvious. But for the urgent call for some working gravitational hypothesis of a mechanical sort to complete the scheme resulting from the new electronic conceptions, I should have no wish to add one more suggestion to the rash, but ambitious conjectures on this subject which have been relegated to the mausoleum for dead whales in the depths of the sea of knowledge.

If the foregoing hypothesis is to be accepted, it seems necessary to distinguish clearly between inertia and mass. An ether-particle may be pulsating like an electron and may thus have mass, but it lacks inertia in its own right, unless vibrating with an oscillatory motion and driven forth with the velocity of light as *luminiferous* ether. Being assumed to possess "mass" in a certain limited sense, we must ask in what way an atmosphere of bound ether can be attached to, or condensed around the atoms. If the bound ether were that of interatomic radiation, cold matter should cease to refract which is not borne out by observation. The presence of bound ether is not manifested except in so far as it is revealed in the refraction of light, where the velocity of light in the denser transparent media is slower because the "bound ether" which interpenetrates their substance and is never absent whether the matter is radiating or not, is denser, being more strongly attracted by their heavier ultimate particles. Thus this shell of ether, while held to the electrons by attraction, acts almost as if emitted by them up to a certain compensating density, and it is only flung off when some sudden change of orbital motion occurs. The supposition that the shell is at once reformed at the expense of the surrounding aura relieves the dilemma arising from its continual loss in the generation of radiation. That there is an intimate relation between the refractive and the electric properties of matter is because ether-corpuscles and electrons are also most intimately related.

Nothing definite is known at present as to the nature of static electric charges upon a solid body, though much is known about the electrification of a gas. The beautiful curled filamentary structure of certain high-tension electrical discharges in air is presumably connected with the viscosity of highly heated gases and a tendency to the formation of complex aerial vortex-rings. The testimony of the electrical stress-figures in resin obtained by J. W. Swan,* where the positive stresses simulate motion under attraction from every direction towards a center, and the negative stresses suggest an attempt at the formation of vortex-rings, or outgoing explosive puffs in a resistant medium, can be interpreted on my present hypothesis to mean that the electrons have poles of different sorts;

**Proc. Roy. Soc. London,* Vol. LXII, p. 38, 1897.

that there is an indrawing of aura through a central core at one pole and outflow at the other pole; that in positive electrification there is a superficial electric shell in which the electrons are so placed as to have their indrawing poles pointing outwards, while in negative electrification the opposite poles are on the outside and there is outflow towards the exterior; and that thus the distinction between positive and negative electrification is that between right-handed and left-handed rotations in the superficial electrons which give the charge. Though the electrons forming the static charge are exceedingly few compared with those making the mass of the solid body, yet the electrical attractions of the charge are comparable with the gravitational attractions of the entire mass.

Definite Statement and Illustration of the Electric Nature of Matter.—As a basis of atomic structure, let it be assumed, then, that the atom is formed wholly of rotating, revolving, and pulsating electrons, and that there is but one kind of electron, which consists of a definite volume of the universal medium in rapid rotation. This rotation of the electron can be viewed in either of two aspects; and two right-handed whirls with their equators in one plane will repel each other if, when moving in opposite directions, they meet and are forced into contact, thereafter assuming opposite directions of motion at right angles to the original line of approach. But right-handed and left-handed whirls, that is, electrons which have opposite rotations, can have their equators juxtaposed in the same plane and will mutually attract each other. It follows that there are *two electricities* which may be formed the one from the other by a simple gyroscopic uptipping (at least in theory, whatever may be the obstacles to the change); and thus, in order that there may be stable equilibrium, the positive electricity in an atom must be closely conjoined with an equal amount of negative electricity; for as soon as one of the corpuscles of the atom is set free, it becomes indistinguishable from any other electron, whether coming from a positive or a negative source. The loss or gain of even a single electron destroys electric equilibrium and converts an atom into a positive or a negative ion with strong chemical affinity.

Next, since we have adopted the theory that the mass of the atom is wholly electrical, it follows that a hydrogen atom consists of 1,844 electrons,[*] this being the ratio of the mass of a hydrogen atom to that of an electron.

According to Bohr's theory,[†] a hydrogen atom consists of a single satellite electron in orbital revolution around a central nucleus which has a positive charge, and with a frequency of 3,290 million million revolutions per second[‡] given by Lyman's measurement of the shortest wave-length in the hydrogen spectrum. This may be accepted, but it is not necessary to suppose that the nucleus is simple. According to the preceding supposition, the nucleus must consist of 922 electrons rotating so as to give positive electric charge (if they were disposed after the manner of a surface charge), combined with 921 of the opposite rotation which would give negative electrification if similarly disposed, so that the two opposite charges existing in potency are almost exactly neutralized, but with a small positive residual which is the equivalent of the negative charge of the satellite.

If the satellite revolves just outside the equatorial circle of the nucleus, the positive charge of the outer layer prevails sufficiently to direct the orbit of the satellite and to keep it in the equatorial plane; but stability requires that the satellite's rotation shall assume the relation of negative electricity to the outer shell of positive electricity and that the direction of revolution shall be the same in both. They would also be true if the extra electron were inside the atom. All of the revolutions must be congruous to avoid eventual disruption.

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[*] In his book, "The Universe and the Atom," Marion Erwin has given an ingenious but complicated scheme of an atom which accounts for the Balmer series in the spectrum of hydrogen very well, but demands as a corollary that the hydrogen atom shall contain precisely $(12)^3+4=1732$ electrons. Millikan's latest result for the mass of the electron, which has been obtained by methods of extraordinary accuracy, definitely condemns this hypothesis. Moreover, by an application of Kepler's laws to the electronic revolutions in the atom, Millikan derives Rydberg's number N, or the fundamental frequency constant, with an accuracy of one part in one thousand, and thus not only gets the Balmer series, but explains it in a remarkably simple way.

[†] *Philosophical Magazine,* Ser. 6, Vol. XXVI, p. 1, July, 1913.

[‡] See R. A. Millikan, *Science,* N. S., Vol. XLV, p. 327, April 6, 1917.

The satellite is relatively free, and it is the one electron which may be easily displaced and lost to the atom, when the nucleus thus stripped becomes a positive ion. The revolving electron which has become free is, I repeat, like any other electron, simply because it is emitted from what we have agreed to call the negative pole of a source of electric current; but actually it has electric sign solely from its association with other electrons. This becomes evident if we trace the consequences of a meeting of a nominally "positive" ion which has lost its satellite, with a neutral atom. Immediately, if the revolutions of the electrons are congruous, the satellite of the normal atom is shared by the two nuclei and serves to bind them together into one molecule, forming the single *bond* of chemical union. The normal, or neutral, atom behaves as if it were negative in respect to the positive ion and there is the electrical attraction of chemical affinity between them; though, but for the shared electronic satellite, the two nuclei would be mutually repellant from the similarity of their electric charges.

If a normal atom meets with another normal atom having its orbital electronic revolutions facing the same way, can the two unite into a molecule with *two* satellite electrons either satellite being displaced towards the intermediate plane parallel to the two equators, and perhaps coming into equilibrium on opposite sides of a common orbit? Apparently this is possible, but with diminished affinity owing to a divided attraction.* Two normal hydrogen atoms with congruous electronic orbits, placed on either side of a hydrogen ion, make the unstable molecule (H_3) having two satellites, for whose existence under favorable circumstances there is satisfactory evidence.

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*If H_+ represents an atom of hydrogen which has lost a satellite electron, and H_- represents a hydrogen atom which still retains its extra negative electron, the constitution which Bohr attributes to the hydrogen molecule, namely, H_-H_- (or the above problemati-

Probable Explanation of the Thermal Periodicity of the Elements.—The theory of complex concentric vortices, elaborated by Hicks, gives a possible explanation of some of the thermal properties of the elements; but it rests upon the hypothesis of a continuous irrotational fluid whose vorticity changes at certain concentric surfaces within the atom, and takes no account of discrete electrons, each a precise replica of all the others. In the effort to see whether the conception of Hicks might not be adapted to the new knowledge I was led to consider that a group of electrons of the same name must tend to expand into a shell by mutual repulsion of the constituents of the group; yet this shell might be restrained from dissipation by further expansion, if it were conjoined with a concentric shell of opposite electrification. Moreover, such a structure may be repeated several times, thus giving the several repetitions in the thermal properties demanded by the thermo-chemical relations of the elements. To hold such

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cal molecule), is hardly likely to exist permanently according to accepted electro-chemical doctrine, because the two negative electrons repel each other and tend to split the molecule. But the molecule, $H_+ H_-$ should be entirely stable, and it serves the purpose of Bohr's theory equally well if we suppose that, in the ionization of the molecule, the negative atom alone loses its extra negative electron by the step-to-step process of orbital shifting, becoming in this way similar to its companion atom. If a single electron (e) is shared between two atoms, the successive stages of ionization are:

$$H_+H_- = H'H = H,\ H' = H,\ H,\ e.$$

The intermediate stage gives no spectrum, but the last or actively transitional one gives the Balmer Series. It is also probable that under certain rare conditions, the hydrogen atom may retain two negative electrons and give the series of lines in the spectrum of V Puppis.

an assortment together as one piece in spite of the tremendous energies demanded by internal revolutions, is not a simple proposition; but possibly the thing may be accomplished if we postulate a medium back of the electrons with a complex vortical flow, somewhat analogous to the Hicks plan, which controls the electronic motions. It is necessary to assume that at each reversal of vorticity, there is an uptilting and reversal of spin in the electrons, considered to resemble gyroscopes, if the expansive force acts at right angles to the directive force. According to gyroscopic analogies, the electrons can rotate either right-handed or left-handed with equal facility as long as the plane of rotation is normal to the direction of revolutionary motion which is that of the directive force; but if shifted radially in the instantaneous direction of the plane of rotation, the electron so urged is liable to be overturned as in a well-known gyroscopic experiment. The reversal of the electron's rotation requires the expenditure of one of Planck's units of work.

Of the hypothetical system of concentric vortices which has been mathematically computed for an assumed irrotational medium, Hicks himself says: "If vortex atoms are realities, the exact quantitative theory developed in this paper can not accord with actual facts, because it is developed with reference to a surrounding irrotational ether, which can not be the case in nature. Nevertheless, many of the general properties would doubtless be similar."*

In the atom which we are considering, the stream lines are "lines of flow" *of the electrons,* but not of their constituent medium, because the components of electronic rotation, or spin, do not vanish as they are supposed to do in irrotational motion, and stream and vortex lines can not "fold together, cross, and expand" in continuity at a limit. Still, even though the gyroscopic reversals are sudden, there is a decided analogy to the condition postulated

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*"Researches in Vortex Motion. Part III. On Spiral or Gyrostatic Vortex Aggregates." By W. M. Hicks, F.R.S. *Proc. Roy. Soc. London,* Vol. LXII, p. 335, February 3, 1898.

by Hicks. I will merely note that the alkali metals which are placed at the maxima of Lothar Meyer's curve of atomic volumes, or in positions which, according to my supposition, mark strong intensification of those thermal properties which are given by nuclear constitution, are of all the elements those which are most prone to give a mixed flame spectrum in which the bright lines are accompanied by a strong superposed continuous spectrum, and they are *thermally expanded atoms*. These atoms are therefore at nodal points of the interior organization of their nuclei, each corresponding to the successive addition of a new pair of electronic shells in the order of increasing steps in the atomic weight.

Professor W. M. Thornton* finds that "the periodic curves of density and atomic volume both have the inflection characteristic of hysteresis. They can be built up on the assumption that the internal force by which atoms are held together passes through a simple periodic change and that in the resultant change of atomic volume there is structural hysteresis." Hydrogen, boron, aluminum, copper, ruthenium and osmium, which are the densest atoms of their respective thermal periods, have their atomic weights proportional to the cubes of the first six natural numbers. Presumably, the most complex atom, uranium, has six rings of supernumerary electrons, arranged in one orbital plane, and six pairs of electronic concentric spherical shells, or what may be likened to a twelve-storied structure, and it is doubtful if any larger number of stories can exist in an atom.

The thermal relations of the atoms appear to be those of their central nuclei, both as to chemical properties and as to those radiations which are especially caused by heat. Perhaps this is what Swedenborg glimpsed when he assigned heat to a "central motion of particles."

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*"The Curves of the Periodic Law," *Philosophical Mag.* for July, 1917, p. 70.

THE LUMINIFEROUS ETHER: (II) ITS RELATION TO THE ATOM

The particles of ether may be produced out of the aura by some modification, perhaps involving a virtual condensation of the aura; but the process has not reached any final limit, for the ether-particles remain highly elastic, and if there is a shell of ether around each atom, it is compressible since the density of the bound ether in matter varies. It may be admitted that we do not know just what ethereal "density" means, and that it is probably a concentration of some electrical property. Just as spongy platinum, by the powerful attraction of its atoms and the large surfaces of its porosities, condenses gases in its pores, and thus by its presence has the same effect as an increase of pressure and favors chemical union of certain gases where this union is facilitated by pressure;—so the heavier atoms condense the so-called ether in the interatomic spaces, and this is done so nearly in proportion to the atomic weight that it is possible to predict the refractive index with approximation from the known atomic constitution of a substance in unit volume, though more closely from the electric properties. It is indifferent whether different atoms are chemically united or merely mixed, as far as refractive properties are concerned. Hence the ability to condense ether resides directly in the atoms. The denser "bound ether" exerts greater electric inductance on the vibrating ether which traverses it and thus retards the passage of light. Though readily thrust aside, the bound ether "particles" can not be dispelled by other particles of their own order of magnitude without suffering some oscillatory disturbance; and this oscillation of the bound ether-particle is itself electro-magnetic and reacts on the electric oscillation of the light-bearing ether-particle* with its reciprocating magnetic vortex in such a way as to retard its onward progression.

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*If we call the direction of advance the "polar" axis of the particle, any oscillation in the equatorial plane would be equally divided between transverse and parallel vibrations in respect to a meridional plane, and polarization in this case would be impossible; but a meridional oscillation may be either transverse or parallel to a given meridional plane. The fact of luminous polarization there-

Since oscillating or rotating ether not only possesses ethereal mass, but also electro-magnetic inertia, the ether, considered as an interatomic medium, is a substance which offers no resistance to the mechanical motion of atoms which do not set it rotating, or oscillating in the special luminous mode, but possesses electro-magnetic inertia for luminous motions which cause its particles to oscillate luminously. Similarly, though the advance of a broad surface composed of many molecules with their atoms and electrons can not cause a single ether-particle to rotate (because the dimensions of the two are on a totally different scale of magnitude), motions of individual electrons which are of a like order of magnitude with the ether-particles and which are not self-compensatory, are competent to impart to the ether temporary electric inertia and momentum. Thus the supposed ether, though having no inertia of its own, is bound to matter by having imposed upon it a portion of the inertia of electronic movement sufficient, at any rate, to cause some adhesion of the bound ether to material particles.

There is also some temporarily associated ether of another sort, for even in solid bodies at ordinary temperatures, there is, in addition to the thermal energy of molecular vibrations mechanically transferred, some low-temperature radiation between the molecules; and as I have shown elsewhere in treating of gaseous radiation, this internal radiation becomes of exceptional prominence and importance in the gaseous atmosphere. To this extent, at any rate, even the radiant ether may be bound to matter.

Various theorists have speculated as to whether the light-bearing ether does or does not possess mass. Clerk Maxwell has assured us that light has momentum and

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fore decides that the vibration of the polarized light is a meridional oscillation. The plane of vibration may, however, be magnetically rotated in an equatorial direction as in the Kerr effect.

therefore the ether must have both mass and inertia as long as it transports light; and he even went so far as to assign to the free ether a definite density in* the ordinary sense.

Luigi d'Auria,† considering the extent of space and assuming it to be filled with ether, thought that the gravitative action of the whole ether would suffice to account for stellar motions; but since he enormously underrated the immensity of space (in confining it to a radius of about 2,000 light-years) and also misunderstood Maxwell whose ethereal density was referred to water as unity and not to air, d'Auria's computation is of no value. Maxwell's density also contained a small numerical error, as I pointed out in my "Cosmic Cycle"‡, but this is a mere bagatelle compared with the gratuitous assumptions which he has made in getting any density at all of the material sort for the interstellar medium.

G. A. Hirn, in his "Constitution de l'Espace Celeste," has given elaborate computations of the action of a resisting medium upon the motion of the planets and has demonstrated that the large material mass assigned to an interplanetary medium by Siemens and others, is an impossibility. But the conclusion that the light-bearing ether has no mass does not follow· The work of Hirn, however, proves that an ether possessing such *ethereal* mass as is required for the various ethereal functions does not fill all space in the form of a continuous medium as was formerly supposed. Whatever ethereal atmospheres there may be are strictly limited to the immediate vicinity of massive bodies. An atmosphere of free ether particles, since these have mass but not inertia, might be expected to follow a planet, but not to share in its rotation. Such an atmosphere might be somewhat more extensive than the atmosphere of air.

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*In his article "Æther" in the 9th edition of the Encyclopædia Britannica.

†*Journal of the Franklin Institute,* October, 1897.

‡*American Journal of Science* [4] Vol. XIII, p. 192, 1902.

This doctrine of the limitation of ethereal atmospheres to spheres greater than the aerial, but still confined to the neighborhood of the heavenly bodies, was held by Swedenborg, was formerly peculiar to him, and probably contributed to the rejection of his theory of the luminiferous ether for nearly two hundred years, because, at least during the Nineteenth Century, it was accepted almost as an axiom that light is propagated through a *continuous* medium filling all space, and by transverse waves in this medium; and therefore, since the stars send us light, it was supposed that the luminiferous medium must extend beyond them. We need not abandon the idea of a universal interstellar atmosphere, but it is *not the atmosphere which conveys light.* The new doctrine of discrete light "quanta," though it may be non-committal as to the precise nature of the luminous entity, rejects the supposed continuity of a luminiferous medium, but harmonizes well with Swedenborg's definition and with the function which he attributed to the ethereal particle.

The revival of a modified corpuscular theory of light no longer compels us to explain the aberration of light on the undulatory theory by "the rather startling hypothesis that the luminiferous æther passes freely through the sides of the telescope and through the earth itself";* and while the meaning of the Michelson-Morley experiment has been much debated, its result is at least in harmony with the supposition that a shell of bound ether is attached to, and is carried along by every atom and thus, by summation, by the earth as a whole in its motion (though not as a rotating external atmosphere which, however, does not enter into the light equation); but this bound ether is not the ether which conveys light from outside sources. This follows also from the result of Lord Rayleigh's inquiry: "Does Motion through the Æther cause Double Refraction?"† confirmed with even more delicate precau-

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*G. G. Stokes, *Philosophical Magazine,* [3] Vol. XXVII, p. 9, July, 1845.

†*Philosophical Magazine,* [6] Vol. IV, p. 678, December, 1902.

tions by D. B. Brace.* These experiments show that the ether concerned in refraction is bound to the earth and travels with it through space.

As Stokes pointed out in the above paper, the adoption of a corpuscular theory of light makes the aberration of light a very simple thing, while the hypothesis of luminous transverse vibrations in a quasi-solid and yet fluid medium, puts a heavy strain on our credulity.

Foucault's experiment which showed that light travels more slowly through transparent bodies of greater density, than it does through bodies of feeble density, demonstrated that the density of the included ether must vary through a considerable range according as it is associated with dense or with rarified matter. This "density of the ether," however, does not refer to that of the particles themselves. They may be not only elastic, but expansile and contractile, though through a very minute range. As light-quanta in free space, they presumably have the same size as electrons, according to the hypothesis of their origin which has been presented in this paper, though the electron is rendered rigid by its rapid axial rotation, while the non-rotating ether is yielding.

Fessenden, assuming that the electronic charges are the sole cause of the mass, and their motions, of the inertia of matter, calculates from J. J. Thomson's formula for the electrically produced inertia of a charged sphere, that the electron is "about $\frac{3}{4} \times 10^{-13}$ cms.† in diameter. The ionic equivalent being about 4 (± 1) $\times 10^{-10}$ e. s. units [now more accurately fixed by Millikan at $e = 4.774$ ($\pm .005$) $\times 10^{-10}$] we find the electrostatic tension and pressure at the surface of the corpuscle, about 2×10^{32} dynes."‡ The revised measurement of the electron's radius makes this more nearly 10^{33} dynes. With this relatively enormous pressure at the surface of the electron, it is necessary that the volume-

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***Philosophical Magazine,* [6] Vol. VII, p. 317. April, 1904.

†J. J. Thomson gives for the radius of an electron "about 10^{-13} cm." (Conduction of Electricity through Gases. 2d Edn., p. 655). Millikan (The Electron, p. 251) gets for this radius, 1.9×10^{-13} cm.

‡*Science,* N. S., Vol. XII, p. 743, November 16, 1900.

elasticity shall be as great as 10^{73}, if the electron is not to collapse entirely; and this represents the electronic volume-elasticity *resulting from the elasticity of the aura.* It is by virtue of this wonderful, almost infinite elasticity that the aura is able to vibrate gravitationally with a periodicity comparable to that of light, through space of galactic dimensions.

The force required to produce the electric condensation at the surface of the electron must be balanced by the equal internal spring of the electron's volume-elasticity. The numerical ratio of volume to surface being that of the cube to the square, and since the surface of an electron is about 10^{-25} sq. cm., we have approximately

$$\text{volume-elasticity of electron} = \frac{(\text{surface pressure})^{3/2}}{\text{surface of electron}}$$
$$= \frac{(10^{32})^{3/2}}{10^{-25}} = 10^{73}.$$

Professor Fessenden suggests that the size of an electron is determined by the fact that the breaking strain of the ether is reached at this point, so that any further electric condensation is impossible. I would also suggest that probably the rotation of the electron at its surface boundary attains the speed of light, and that this is why when the electron is stript of its superficial layer of ether, the latter is flung off with the same speed. Also that the velocity of electronic rotation is the same at all points of the surface of the particle, so that the angular velocity increases from the equator to the poles, being

$$\frac{2.999 \times 10^{10}}{2\pi \times 1.9 \times 10^{-13}} = 1.660 \times 10^{22} \text{ revolutions per second}$$

at the equator, if we adopt Millikan's value of the radius of the electron. This is about 1,000 times as great as the frequency of the characteristic vibrations of the lighter atoms. Hence the angular velocity of the orbital revolutions of these electrons is presumably less than that of their rotations.

The configuration of this superficial electronic motion is similar to that which Swedenborg assigns to his elementary vortical particles (mentioned *ante* p. 9). An example of a particular case of such motion may be seen in my notes to the new English edition of the *Principia.** In his doctrine of forms Swedenborg describes a fifth natural form, superior to the vortical, "by which one thing regards another as well as itself, nor is there anything but what consults the general strength and concord."† On the lowest plane of nature, this form exhibits itself in the gravitational wave by which each particle is united and consociated with all the particles in its galactic cell. Thence comes the interaction and universality of natural law. Nature is not diverse, here of one sort, but elsewhere totally different and unrelated. Nature is one and the same throughout her vast complexes, and this unity appears to require some such fundamental singularity as that of the form of motion and the size of the electrons which must everywhere be precise duplicates.

The origin of a light-quantum I conceive to be the gyroscopic uptilting and reversal of an electron, accompanied by reversal of electric sign. If the center of rotation of a gyroscope is revolved in the direction of no resistance, there is no uptilting; but revolution in the opposite direction experiences resistance which reverses the spin. Similarly, a rotating electron may be carried around an orbit and brought back to its original position without performing work; but to reverse the plane of the electron's rotation requires the expenditure of one of Planck's least quantities of energy, and this means that resistance has been encountered. The quicker the reversal, the more intense is the radiation; but the period of the vibration appears to depend on some function of the radius of the electronic orbit. The simplest supposition is that the

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*Appendix A. Vol. II, Fig. 4 *a*, p. 623.

†For illustrations of this see my paper in the *New-Church Review,* Vol. XIX, p. 256.

radiant period is the same as that of the electronic revolution, but this does not necessarily follow.

Where many electrons are closely associated, it is difficult, or almost impossible to overturn a single electron without at the same time effecting a similar overturning in a multitude of others; but the few electrons whose group is denoted by the Moseley number, are relatively free. Each executes its own period, though, even here, the mutual relations of attraction and repulsion which tend to preserve stable relative positions, can not be disturbed by the misplacement of a single electron without starting a series of vibrations which give complicated line-spectra through the complex perturbations of the group. Ionization of the atom is an inevitable concomitant of the operation, that is, the radiating atom either loses or gains one or more electrons.

The adoption of the assumption that the electron represents the greatest possible condensation of electric force, and that there is only one kind of electron, along with Fessenden's conception that its size (and therefore its electric charge) is conditioned by the breaking strain of the ether, leads readily to my thesis that the electron has a rotation and a peculiar form of vortical motion similar to Swedenborg's elementary particle, and furnishes a reason why it should have this peculiar form. In fact, no other form would fit the demand that the surface shall everywhere have the rotary velocity which is fundamental in the physics of the ether and definitive of the relation between electrostatics and electromagnetics. My further proviso that an ethereal electronic sheath is cast off under shock at the moment of reversal of electric sign, which reversal is the origin of the reciprocating magnetic field of the luminous particle, and that the sheath becomes an ether-particle having the same significant rectilinear velocity as the circulatory velocity of the electron's surface, also accounts for the fixed value of the energy-constant, equivalent to the light-quantum, and defines it as the energy required to overturn and change the sign of an electron.

The same is in perfect agreement with, and satisfactorily interprets the well known law that "positive and negative electrical charges always appear simultaneously and in exactly equal amounts"; for if positive and negative electricity are but two different aspects of one and the same electric unit, the separation of a pair of electrons must inevitably give equal and opposite electrical charges.

Though not expressed in identical terms, my supposition that Planck's least quantum of energy represents the resistance which an electron offers to a gyroscopic uptilting with reversal of electric sign, agrees in a general way with Fessenden's hypothesis that Planck's "*h*" represents a gyroscopic reversal of vorticity.*

Swedenborg had the idea of "small corpuscles which float in the ether and continually issue from the hard body; thus the ether is urged into a whirling motion at a distance from its body, and this causes light, and to a certain degree, electricity. For no bodies exist which are not in some way or other penetrated by the ether."† Though somewhat vaguely stated, if the "whirling motion" is thought of as a vortex filament in the *magnetic* medium, and the "corpuscles" as Thomsonian corpuscles, or electrons, this corresponds to the present conception of a static electrical charge on the surface of a "hard body" with its attendant Faraday "lines of force." Swedenborg's version, that "in order for anything to be electric and attract very light bodies, a certain circular motion is necessary in the ether," also accords with Sir J. J. Thomson's interpretation of an electric line of force, namely, that electric attraction is to be ascribed to the tendency of vortex filaments in an intervening medium to shorten themselves.

Benjamin Franklin, according to Millikan ("The Electron," p. 15), was the first to express his belief in the existence of a definite electrical particle, where he says: "The electrical matter consists of particles extremely subtle,

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*R. A. Fessenden on "Gyroscopic Quanta," *Science*, N. S., Vol. XXXIX, p. 533, April 10, 1914.

†*Principia*, new English edition, Vol. II, p. 221.

since it can permeate common matter, even the densest, with such freedom and ease as not to receive any appreciable resistance." I have not been able to verify this quotation in any work by Franklin to which I have access. In his note on "Protection from Lightning," written at Paris in September, 1767, Franklin says: "The matter of lightning, or of electricity, is an extreme subtle fluid, pentrating other bodies, and subsisting in them equally diffused." This says nothing about electric particles.

Swedenborg, at any rate, declared that the light-bearing ether was made of corpuscles and associated them in an indefinite way with certain other "corpuscles" which somehow were connected with electricity. This is exceedingly vague, but in my view it contains an adumbration of the truth. Meanwhile, in the gradual dawn of the scientific day, Swedenborg's "corpuscles" and Franklin's particles of "electric matter," more subtle than common matter, have become the "electrons" of Johnstone Stoney, and the "corpuscles" of J. J. Thomson; and at the hands of the latter investigator vague hypothesis has given way to experimental demonstration and exact quantitative measurement.

Is Entropy always One-Sided?—For the last half century it has been recognized that a certain sum total of energy may be divided into two parts, namely, the free or available energy, and the entropy; and that the available energy of sun or earth is continually diminishing, while more and more of energy escapes into the aura, there to be stored up as entropy which, according to Clausius, is a continually increasing quantity tending to a maximum, while the free energy of every material configuration either remains the same, or diminishes. It can in no wise be increased by human effort.

The thermal mechanism of matter has been likened to a clock which is slowly running down; but as Sir Oliver Lodge has said: "It would seem as if the second law of thermodynamics must be somewhere disobeyed—at least if the age of the universe is both ways infinite—else the final consummation would have already arrived." This dilemma has been considered in my paper: "What becomes of the Light of the Stars?"* and I think it is now fairly certain that matter is gradually destroyed and converted into ethereal energy of radiation, which, in turn, is gradually absorbed as it passes through the universal medium, or aura, again originating matter in its incipient forms.†

We have then to recognize a reversal of the original entropy proposition, namely, a building-up at this stage, in place of a tearing-down; for how would it be possible for the universe to endure unless both operations were actual? We may admit that in the part of the universe in which we are placed, the tearing-down operation is going on; but it follows on any rational supposition that there must be other parts of the universe where the building-up of a renewed system takes place, where the first rudiments of matter are coalescing and developing into gaseous atoms and molecules, or into meteoritic crystals and are aggregating into nebulæ, stars, and groups of stars, star-clusters and galaxies.

Gravitational Relationships.—This is not the place to discuss at length the origin of matter, but something must be said concerning the gravitational fields of the electrons and ether-particles, which there is reason to suppose are

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**Popular Science Monthly*, Vol. LXXXII, p. 289, March, 1913.

†Mr. Harlow Shapley has announced (*Proc. National Academy of Sciences*, Vol. II, p. 14, Nov. 17, 1915) that the frequency of colors in the star cluster Messier 13 shows conclusively that there is no *selective* absorption of light in space (a conclusion which I had already reached on other grounds in my paper, " What becomes of the Light of the Stars? "). But his further statement that " in the light of this result we are probably justified in assuming that the non-selective absorption in space (obstruction) is also negligible " (*Op. cit.*, p. 15), though it may be justified if by " obstruction " is meant a non-selective cutting out of light by particles too coarse to exert any selective influence, fails to cover the more general non-selective absorption of light by the aura which fills all space. The existence of *this* absorption is shown in my examination of the white nebulæ, " Are the White Nebulæ Galaxies? " quite as strongly as the non-existence of selective absorption is shown by the other argument.

limited to a single galactic cell.* Lodge computes that if there could be substituted for the gravitational pull of the sun which holds the earth in its orbit, an elastic pull through steel rods and within their breaking strength, it would take "a million million round rods or pillars each thirty feet in "diameter" to stand the strain.† This strain is not carried by a narrow beam of aura intervening between the two bodies, but is the resultant of the interaction of fields of force which fill an entire galactic cell. Though seemingly impalpable, the aura is stronger than steel, but this strength results from its intimate connection with an infinite source of energy which is spiritual. So does the invisible spirit control the body; and even so public opinion, a seemingly intangible thing, is stronger than the most powerful autocrat, and in the end will prevail. Because of the universal relationships of the aura, it has the strength of the many in one; and the wonderful thing about the electron, considered as the gravitational unit, is that through its gravitational field of force it is everywhere present (at least within the boundaries of its own aural cell) and receives into itself an impulse from and an impress of all that transpires in a vast galactic environment.

The electrons are like little organisms, or least hearts, whose synchronous systole and diastole make the life of the universe, perpetually receiving and transmitting energy from an inexhaustible source. The aura is the "fundamental substance." As Sir Oliver Lodge says of it in his searching analysis: "It cannot really *be* ordinary matter, because ordinary matter is definitely differentiated from it, and is presumably composed of it; but the inertia of ordinary matter, however it be electrically or magnetically explained, must in the last resort depend on something parentally akin to inertia in the fundamental substance which fills space."‡

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*See my paper: "On a Possible Limit to Gravitation," Publications of the American Astronomical Society, Vol. III, p. 335.

†"The Ether of Space," p. 128.

‡"The Ether of Space," p. 135.

At the center of each aural cell is a galactic nucleus from which issue either two spiral streams of stars, or else concentric spheres of stars with but little spiral structure.

Galactic Dimensions.—In regard to the actual status of the galaxies Mr. Gore has shown,[*] and I have shown independently,[†] that the nebula in *Andromeda* must be an object of truly galactic dimensions. On the supposition that the nova which appeared near the center of the nebula in 1885 had an absolute brightness equal to that of *Nova Persei* 1901, I found that the nebula must be at least at a distance of 1600 light-years. If the star in the nebula had an absolute brightness of the same order as that of Tycho Brahe's star, it may have been still more massive and something like five times as far away as the first estimate, or at 8,000 light-years; but anything brighter than this or any larger distance seems improbable, for *Nova Persei* was itself an object of phenomenal size, for which (in *Astronomische Nachrichten,* Nr. 3771) I deduced a mass 1149 times that of the sun. Mr. Curtis gets 10,000 light-years as an average value for the distance of certain white nebulæ not more than one-tenth of the angular diameter of the great *Andromeda* nebula.[‡] If these objects have approximately the same real size, this would agree fairly with my first value; but my second value gives dimensions to the nebula which are more nearly comparable with those of our own Galaxy. If all the galaxies, including our own, have somewhat similar real diameters, the Curtis nebulæ should be more nearly 80,000 light-years distant, and this seems to me a very reasonable assumption.

Admitting that the white nebulæ are galaxies,[§] or somewhat widely separated aggregations of double spiral form, composed of both stars and nebulous material, and in some cases (as in

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[*] T. Ellard Gore in *Knowledge,* N. S., Vol. 6, p. 247, July, 1909.

[†] In *Astronomische Nachrichten,* Nr. 4536.

[‡] *Publications of the Astronomical Society of the Pacific,* Vol. XXVII, p. 218, 1915.

[§] As is indicated in my statistical study in *Astronomische Nachrichten,* Nr. 4536.

N. G. C. 4594 and 5746) with annular annexes of dark meteoritic material, it becomes of interest to ask whether gravitation also holds sway from one galaxy to another, or whether there is a limit to the extent of the gravitational field around a given galaxy.

Galactic Distribution and Gravitational Properties Which May Be Inferred Therefrom.—Considering that the parallax of the nearest star is almost one second of arc, if we represent the disk of a star 1,000,000 km. in diameter by a little circle of 1 mm., we should have to place a similar dot representing the next nearest star at a distance of $(92.9 \times 206{,}265)/10^6$, or about 192 km. away; and granting that there may be regions in the Milky Way where the star-density is 1,000 times as great as in our neighborhood, yet even there the dots would be almost 20 km. apart. This gives a startling picture of the sparsity of stellar material, and affords very little ground for the supposition that the lucid stars owe their high temperature to collisions with other stars, since the chances of their meeting are almost infinitesimal.

On the other hand, if we let a millimeter dot stand for a galaxy, neighboring galaxies will be represented by similar dots sprinkled only a few centimeters apart; and since these objects have velocities of upwards of 1,000 km. per sec., frequent collisions are inevitable unless there are mutual repulsions which develop upon arriving at too close proximity. This result might, however, be produced in another way, provided gravitational attraction by which we assume the stellar movements are given, is not universal, but is limited in its action to definite volumes around each galactic center, within which gravity is controlled by definitely limited currents of the aura. In fact, the great velocities which are exhibited by certain of the white nebulæ do not necessarily imply that the motions have been imparted by the attraction of masses enormously greater than that of our Galaxy. The entire field of gravitational potential energy belonging to a given galaxy being borne by a limiting volume of surrounding aura, we may presume that if this volume of aura has a motion of its own, the system of enclosed material particles will necessarily follow. The galactic multitude will be carried along as if it were so much floating material borne on a supporting current of the universal medium to which each particle is intimately attached by its very constitution, though the cause of the motion is not to be sought by pressure in currents of immediately juxtaposed aura, but in more extensive aural currents, with which the matter is connected through fields of force. Moreover, as each portion of aura, attendant upon a given galaxy, constitutes, as it were, a single compartment, or cell, in a great multi-galactic organism, while it is conceivable that occasionally a fragment of aura with its enclosed matter may be torn off from a given cell, and may invade a neighboring cell, we might rather anticipate that, as a rule, the individual compartments will retain their individuality. In general, an entire block of aura with its contained galaxy will not penetrate a neighboring block as one mass, but if it must move in that direction, it will do so by thrusting the other to one side. Except for certain gaseous nebulæ of bizarre forms which appear to be entangled among stellar masses belonging to our own Galaxy, the other, or white nebulæ, as a rule, exhibit a general similarity of type; and in only a very few cases is there any appearance whatever of that reckless disarray which might be charged to interpenetrating or colliding galaxies. I think we may infer that in spite of what is geometrically a very close proximity, these galactic masses, while moving among each other freely, do not collide; and if occasionally a fragment of the mass is torn off and sent far afield, together with its enclosed matter, there is produced at most only a lone wandering star, moving with the velocity of the original fragment, which may for a while thread the intergalactic spaces, or even pass through foreign galaxies as a "run-away star," but which will eventually be subdued by the persistent lagging of its attached aura, just as the relics of a tornado go on whirling for a while, but are at last absorbed in the more general movements of the atmosphere.

We are thus led to look upon the galaxies as local knots of aggregation, separated by considerable spaces, but not by spaces which are enormous compared with the galactic diameters; and these intermediate spaces need not be wholly devoid of stars. It has been surmised that some of the faintest stars shown on photographic plates may be at distances of something like 50,000 light-years. This is possible, but if so far away, it is improbable that these stars have any connection with our Galaxy whose condensations occupy a much smaller space.*

Pulsations and Revolutions of Electrons in the Atom.—If the pulsation of the electron involves an elastic reciprocating vibration between the whole vast body of aura in an aural "cell" reacting upon the elastic resistance of the rotating aura circumscribed by the surface of an electron, the superficial condensation need not be great, but at any rate the interior resilience must be enormous. In fact, we should infer as much from the wholly independent result obtained by Fessenden for the pressure at the surface of an electron. The atoms in their most condensed form in liquids and solids have equally an enormous interior resistance and are practically incompressible. This property will have to be referred back to the great resistance to pressure of the constituent electrons.

The Pulsating Electron and Its Relation to the Universe.—According to the experiments of Bjerknes with pulsating spheres, if the results are applicable to gravitation, since negative gravitation is unknown, it is necessary that all of the pulsators shall pulsate synchronously with identical periods and phases. The gravitational pulsation must therefore be the unique origin of time and its most fundamental expression. It also constitutes a most amazing evidence of the absolute unity of the created universe. Hicks

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*As is shown in my paper: "On Stellar and Nebular Distances," *Knowledge*, Vol. XXXV, No. 530, September, 1912, pages 329-332; and No. 531, October, 1912, pages 373-376.

remarks on this point: "All we have to suppose is that atoms pulsate with a constant period, and that none have phases differing by more than 90°, or that if such once existed they have been eliminated." No suggestion has been offered, however, as to any means by which this elimination can be effected.

Now in spite of an infinite diversity, there is in the universe a unity which suggests a consistent plan and universal correlation of parts. Spiritual philosophy attributes this unity to the Oneness of a divine Creator. If the synchronous pulsations of the electrons can be demonstrated, it will be the nearest approach that science has ever made to the acknowledgment by the magicians of Egypt, the very dust having become alive: "This is the finger of God" (Exodus, viii, 19). Verily, the mystery of the infinitely little is as overwhelming as that of the outstretched heavens.

Hicks says: "If the theory offered is the true one for the explanation of gravitation it would be possible to have celestial systems, the parts of which in each would obey the law of gravitation, but which would not influence each other, or would repel each other." Evidence has now been adduced which seems to me powerful in confirmation of this conception. The relative crowding of the galaxies is so great that it argues a mutual repulsion between these larger parts in a system of another order.

Various investigations of the pulsatory gravitative hypothesis have been made, notably those of the mathematicians, Hicks, Burton, and Leahy. In these studies it has been found necessary to assume the existence of points which are sources or sinks of fluid motion. Swedenborg also predicted the beginnings of the universe from "points'" which are centers of "conatus," which may perhaps be translated "energy." But however permissible the assumption of these "sinks" and "sources" may be as mathematical devices, it is impossible to conceive of them as actually existent, except as the outcome of a hyper-space of whose reality several eminent mathematicians have speculated, and for which even a semblance of experimental evidence has been given by Zöllner in his "Transcendental

Physics." The existence of properties which transcend those of matter and tridimensional space is forced upon our attention whenever we approach the boundaries of the material universe.

While the enormous velocity of gravitation indicates that the transmitting medium is almost incompressible, the atom and the electron must be regarded as definitely limited volumes within which a diminution of the "density" of the aura arises through the centrifugal force of a rapid rotation. Burton assumes that "the nucleus of an electron, instead of being vacuous, is merely a region of somewhat diminished density. . . . This assumption as to the nature of the electronic nucleus is admittedly gratuitous, but apart from the difficulty regarding mobility which it was designed to remove, it has the advantage of greatly simplifying the dynamics of the problem proposed." (*Philosophical Magazine,* [6], Vol. XVII, p. 74, 1909). By admitting the existence of local variations of density in a nearly incompressible and highly elastic medium, the transfer of such centers of density-variation becomes comprehensible, and likewise the interaction of atomic pulsations may be grasped.

In order that a mass of matter, that is, a partial vacuity in the aura, may move from position *a* to position *b,* there must be an inflow of aura at *a* and an outflow at *b,* which requires that there shall be a gradient of pressure in the aura between *a* and *b,* and that work shall be performed by the aura at the expense of its potential energy.

Mr. A. H. Leahy has urged an objection against the theory we are considering, and in regard to the proposition "that all atoms are pulsating in phases not differing from one another by more than a quarter period, and that the intervening medium is an incompressible fluid" he says: "If this were the case, the law of universal attraction according to the law of the inverse squares would follow; but unless the medium is supposed to be absolutely incompressible, in which case all pulsations would be instantaneously diffused throughout space, there would on this theory be repulsion between bodies at distances greater than a quarter wave-length, and bodies would at certain distances repel one another, which is contrary to observation."* Since in the present paper, evidence is adduced showing that presumably bodies of galactic magnitude, and separated by intergalactic distances, do repel each other, we have only to predicate of the gravitational quarter-wave a length of the order of the radius of an aural cell, and we shall have a theory of pulsating matter which accounts for both the atom and the galaxy as necessarily interrelated parts. Thus in the light of the new evidence, the supposed incompatibility between observation and theory becomes, on the contrary, a powerful argument in favor of the new theory.

As to the direction of propagation of the gravitational wave, if the direction is strictly radial, it is necessary to suppose that the wave is reflected directly back upon itself at the remote boundary of the cell; or else we must presume that the radial direction is eventually diverted into a circulatory path. The latter accords better with magnetic analogies. In neither the magnetic nor the gravitative fields is there any direct flow of the medium along lines of force as conditioning the potential; but conditions of magnetic polarization, or of pressure strain, are involved, either of them following lines controlled by the peculiar properties of the organ of transmission. Since equal numbers of electrons, having by their positions and rotations the properties of positive and negative electricity, are associated together in the atom, it may be proper to consider the gravitational unit as a pair of electrons, each pair constituting an electric doublet. The immediate electric attractions of the components of the doublet being thus very nearly satisfied, gravitation becomes a secondary residual effect of relatively very inconsiderable importance, compared with the major electrical forces from which the inner energy of the atom springs. Even if independent sources

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** Transactions Cambridge Philosophical Society*, Vol. XIV., p. 61, 1889.

must be assigned to the gravitational and the electrical effects, the latter being especially manifested in chemical affinity, nevertheless, the two are so intimately associated in the atom that they can not be entirely divorced. On the whole, however, it seems more reasonable to attribute cohesion to the attraction of such electric doublets, but to assign gravitational attraction to the interaction of the pressure fields of pulsating electronic rings.

"Bound Ether."—It is now pertinent to explain my conception of the "bound ether" more fully. In the first place, by "ether" in the passage quoted from Sir J. J. Thomson, is not to be understood the specially organized entity which conveys light, but a condensation of the universal aura in the immediate vicinity of matter, which is governed by the fourth-power law appropriate to the fundamental magnetic substance; and it would perhaps be permissible to call it bound *aura,* rather than "bound ether," though if it is desired to look upon the "atmospheric" sheath of the electron as a sort of attached particle which, when detached, becomes an ether-particle, it is also permissible to do that. The ether which conveys light, however, is not so governed or hampered in its progress through interstellar space, but, like the electrons, it is free, corpuscular, and possesses radiant energy which diminishes with the inverse square of the distance as it moves onward with the speed of light, simply by spreading its field of force over a wider area. Inherently, the energy of the ether-particle does not change until it is ready to be reabsorbed, either by matter, or by the universal aura in the reconstitution of matter in its incipient nebular form.

The rays of light are not deflected by passing through the bound ether attached to matter, because they readily penetrate this ether; but they are *retarded* by it through electromagnetic forces developed in the medium. Thus aberration of light is entirely independent of the bound ether, nor can the rotation of a mass of matter impart a like rotation to the aura. The attached ether moves with its attracting matter from one locality to another. This moving attached ether accompanying a transparent body in its motion does not carry with it, control, or push aside any radiant ether which may enter it by the crude mode of a bodily displacement. Yet in a more subtle way, through induced vibrations and the drag which they impose upon the passing rays, the bound ether, whether quiescent or moving (and independently of its state of relative motion or rest) does exert a retarding influence on the passing radiant ether, but one which is determined by the density and electro-magnetic state, and not by the motion of the bound ether. Thence comes refraction.

Is there a More Extensive Atmosphere of Ether Attached to the Earth?—The argument of Sir J. J. Thomson, noted above, which limits the extent of the bound ether shell (consisting of bound aura) to the immediate vicinity of matter, is conclusive as regards an *electronic* atmosphere. Swedenborg, however, imagined that there might be atmospheres of ether encompassing the heavenly bodies and much more extensive than the aerial atmospheres, but also everywhere penetrating between the grosser particles of solid matter. In his futile attempt to improve the Cartesian vortical scheme for explaining the planetary motions, he at first ascribed their cause to the interaction of vortices in these extended spheres of *ether*, though distinguishing the ether from a universal interstellar atmosphere consisting of still finer particles. After explaining that the earth was thrown off from the sun and at first moved near the solar surface and performed "its axillary revolutions more rapidly than it does at a farther distance from the sun, where a considerable portion of it is consumed in the formation of ether, air, water and terrestrial material,"* Swedenborg proceeds† to develop this theory. The correct translation here is a matter of more importance. Clissold's English translation conveys a wrong impression.‡ Rightly rendered,

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Principia, Part III, Chap. XI, n. 2.

†*Op. cit.*, n. 4.

‡Compare the original: "*Principia Rerum Naturalium sive Novorum Tentaminum Phænomena Mundi Elementaris Philosophice Explicanda,*" of which a very carefully edited edition has recently

the text states that the earth, moving among the elementary particles which surround the sun as a medium and fill the whole space of the solar system to the outermost planets, sets in motion, or drives (*agat*) a vortex of its own, which in the early stages of the earth's development was large because the earth then moved more rapidly, and a rapidly moving body can drive a larger vortex than a body which moves slowly. Also a planet of greater mass draws, or retains (*trahat*) a larger vortex, because "the impetus and momentum (*pondus*) are compounded of the mass multiplied into the velocity." This language is applicable to and consistent with no other conception than that of a rotating sphere of bound ether attached to the earth and retained by ethereal friction. Swedenborg supposed that a similar, but immensely greater sphere of bound ether surrounds the sun, its volume having some relation to the *mass* of that body. Since the lesser volume moves in the greater, the two necessarily react.* It was natural to inquire whether the reaction of these vast volumes of bound ether might not explain the planetary motions. The conception, though evidently suggested by the vortical theory of Des Cartes, is a great improvement on the latter in some respects and much more subtle; but it can not be admitted. The phenomenon, if it were possible, would have an analogy to the invisible support of a soaring bird whose momentum is transferred through aerial viscosity to an immense volume of air, and the weight of the bird by innumerable intermediates rests upon a wide surface of the earth.† It would be inconceivable that a current of ether, circulating around the sun and of no greater section than the earth's, should exercise any immediate directive influence on the earth's motion; but it was plausible that the in-

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been brought out by the Swedish Royal Academy of Sciences in "*Emanuel Swedenborg Opera quædam aut inedita aut obsoleta de Rebus Naturalibus,*" Vol. II, Cosmologica. Stockholm: 1908.

* As shown, *Op. cit.*, n. 5.

†See my paper on "The Rôle of Viscosity in Air Support of a Moving Aeroplane," *Technology Quarterly*, Vol. XXI, p. 490, Dec., 1908.

teraction of immense volumes of ether might bind together their respective centers through ethereal viscosity. Swedenborg's design of a flying machine shows that his thoughts had been directed to mechanism of flight and that he had grasped its essential principles. Here, however, analogy failed. The volumes of bound ether accompanying sun and planets, if retained by friction, will be conditioned by the areas and the squares of the velocities of the frictional surfaces. Hence, since the sun's equatorial velocity of rotation is only 4 1/3 times that of the earth and 1/6 of Jupiter's, the sphere of bound ether surrounding the sun, if it were thus retained, could not extend much farther than that of some of the planets. The supposed interaction of the solar and planetary vortices simply does not exist. When it is recognized that a deep layer of bound ether can not be held by friction, the conception of extensive ethereal atmospheres and of their interaction *as a source of gravity* must be abandoned; but the germ of the idea of gravity conceived as an interaction of interpenetrating fields of force in *one* universal atmosphere is contained in these early vortical theories. Whether there may be an earthly atmosphere of free ether-particles which do not move with the velocity of light, but remain at rest in respect to the earth and therefore are not like ordinary ether, and which outreach the aerial atmosphere, and what the function of such an atmosphere may be, must be left undecided; but the functions are not gravitational. If there be such an atmosphere, it is perhaps the physical basis of exterior thought, which may be said to be "in space," and especially of that telepathic thought which is received by harmoniously attuned instruments.

Later on, this theory of gravity appears to have been given up. Its author still holds to the discontinuity of the ether and its existence as a distinct, though not a universal atmosphere; but he assigns the gravitational rôle to a more subtle atmosphere which extends throughout the celestial spaces: "The three natural atmospheres originating from the sun of the world, are the purer ether, which is universal, and from which is all gravity; the middle ether which makes a vortex about the planets, in which also is light, in which are the satellites, and from which comes magnetism; and the ultimate ether which is the air." This conclusion, though not reached until near the end of his life and not published until after his death, was not explained at length; but it seems to have been deliberately and carefully thought out, and it is in several respects in accordance with present ideas. If Thomson is right, the sphere of bound ether can extend little farther than the air, and can not include the satellites, since it must not be confounded with the planet's magnetic field.

In an intermediate stage of his thought, Swedenborg considered the possibility of *four* atmospheres, two of them, respectively assigned to gravitational and magnetic activities, being described as universal, but distinct. In his last view, he allows the ether to exercise both electric and magnetic functions and omits the purely magnetic atmosphere. This is in accordance with the salutary rule that it is undesirable to multiply agents where a single one is competent to fill several offices; and a while ago a similar dual, or "electro-magnetic" wave-theory in a single "universal" ether was in vogue. But today, in view of the greater penetrative power of the magnetic field, and the decidedly electrical affinities of the ether, it is in order to assign the dual function of both gravitational and magnetic activity to the universal atmosphere, while grouping electricity and ether together as being two species of an entity always most intimately associated with matter, save when in transit from one part of space to another after having been ejected from its original material source. Both the free electron and the vibrant ether-corpuscle, the one in the lightning-flash, the other in the sunbeam, have been violently disrupted from their association with matter and sent speeding through space; but they had previously existed *in* matter, and, indeed, are its originating constituents. In a word, all matter may be said to be formed out of light in the depths of space.

Sir Isaac Newton formed, but did not publish, a broad general conception which is somewhat allied to the theory presented in my paper: "What becomes of the Light of the Stars?" He thought that there must be some sort of interstellar atmosphere of an attenuated substance, the "food of the sun and planets. . . . Thus perhaps the whole frame of nature may be nothing but various contextures of some certain ætherial spirits or vapors, condensed as it were by precipitation";* but this substance was not our so-called ether, nor did he attempt to define either the nature or the activities of this universal medium. His "light-corpuscles" were in no sense the components of a universal atmosphere.

Swedenborg went much farther and labored strenuously at the herculean task of devising a system of interrelated media whose particles should be constituted on mechanical principles, while answering at the same time as a basis for various physical forces. Mathematically, he was poorly equipped for the solution of the problem, but as a philosopher he had no superior, and some of his results are in extraordinary agreement with recent scientific discoveries. Such are his ascription of luminous and electric functions to the ether, and his teaching that the ether is not the same as the interstellar atmosphere; and though he was on a wrong track in supposing that gravity results from the interplay of limited vortices of ether attached to sun and planets, he approached the truth in another way; for the electron which is so closely related to the ether, is now seen to be the first gravitational unit; and even the idea that gravity is produced by the mutual pressure of interpenetrating vortices has a foundation if the theory be transferred from the ether to the aura, or to the "purer ether" of the preceding quotation, and if the vortices are given galactic dimensions. In fact, though I arrived at my conclusion in another way, I now see that eventually Swedenborg probably reached a somewhat similar general conception (in the posthumous passage already quoted).

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*Newton's Letters, etc. *Philosophical Mag.* (3) Vol. XXIX, p. 190, 1846.

Professor R. W. Brown* concludes that Swedenborg was probably misled into his error of supposing that there are solar and planetary ethereal vortices of opposite sorts which act and react, through his having noticed that the satellites move faster than the surfaces of their planets, but that the planets revolve in their orbits more slowly than the sun's superficial rotational velocity. Instead of attributing this fact to the action of two kinds of vortices, one moving swiftest at the circumference, the other swiftest at the center, it is more reasonable to see in this a reminiscence of the past history of these bodies, and to infer that the planets have developed from the sun successively, or from within, and show their relation to the central body of their parent and to his waning powers in this way; but that the satellites have come from without, thus by capture, and exhibit a wholly different relation.

In further elucidation of this subject, may be mentioned a passage from Swedenborg's "Economy of the Soul's Kingdom" (Vol. I, n. 299): "There are three species of motion, namely: (1) *local* or translatory motion; (2) *undulatory* or modificatory motion; (3) *axillary* or central motion. There is, moreover, (4) *animatory*, or alternately expanding and contracting motion; and to this may be added (5) *conatus* or effort, which is a perpetual tendency to motion."

The separate consideration of "three species of motion," and then the addition of yet a fourth kind, indicates that the latter is thought to be peculiar, or somewhat apart from the obvious sorts of motion. This would apply precisely to the pulsating movements of the electrons from which gravitation presumably arises. These movements can never be observed directly, but must be inferred from their effects.

The final mention of a fifth species which seems to be identical with the most modern conception of what we now call "energy," leads to the recognition of a variety of hidden, interior, or "potential" movements which also have to be inferred

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*In *The Journal of Education*, published at Bryn Athyn, Pa., Vol. XV, p. 148, April, 1916.

from what they do. It is by the eye of reason that we must penetrate more deeply into the hidden recesses of nature.

Having now viewed our subject from various sides, we are in a position to say that, according to present light, the free ether consists of spherical elastic particles, while the electrons are cored vortices whose rigid surfaces are everywhere rotating with the velocity of light; that the ether, in so far as it is atmospheric, appears to consist not so much of a single atmosphere, but rather of innumerable condensations of the universal atmosphere around the electrons from which it is thrown off in radiant emission; that the ether penetrates the structure of all material forms, but with diverse degrees of freedom according to electrical properties, and is everywhere closely associated with matter, sharing with the electrons in a gravitative pulsatory motion of its individual particles by which both are continually connected with the source of their sustenance in the universal aura, and having besides, when free, a peculiar oscillatory motion of its own; that in this way the tiny electrons are most intimately conjoined with the vast galactic spirals and like them endure for ages, while the light of the stars, though dimmed, is not extinguished after traveling for millions of years; but as light-bearer, the ether is not an atmosphere, but an emanation of discrete particles, images of vibrating electrons, involving in their inception least quanta of energy, formed out of the electronic or intra-atomic "atmospheres," and possessing innumerable varieties of specific vibrant form which reflect the electronic motions at epochal moments of perturbation and inversion, and which are accompanied by electromagnetic fields of force in the universal aura. These extended fields interpenetrate and sometimes interfere. This is an apparently simple statement, but without the discussion which precedes, it would be unintelligible.

Westwood Astrophysical Observatory,
Westwood, Massachusetts,
May 27, 1918.

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THE RADIANT PROPERTIES OF THE EARTH FROM THE STANDPOINT OF ATMOSPHERIC THERMODYNAMICS

BY
FRANK W. VERY

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EARTH RADIATION AND ATMOSPHERIC THERMODYNAMICS

By Frank W. Very

LUNAR AND TERRESTRIAL ALBEDOES

THE LUMINIFEROUS ETHER

A PREVISION OF SCIENTIFIC PROGRESS

BEING A SCIENTIFIC ESTIMATE OF SWEDENBORG'S PRINCIPIA

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HISTORICAL NOTE

THIS paper was read in abstract at the nineteenth meeting of the American Astronomical Society at Swarthmore College, September, 1916 (Publications of the Society, Vol. III, p. 232). The statement that the radiant properties of the Earth are "mainly those of its atmosphere", which is made here quite definitely, is contained in embryo in the author's treatise on "Atmospheric Radiation" (*U. S. Weather Bureau, Bulletin G,* 1900), and in his various papers on atmospheric transmission, as, for example, in the one on "The Transmission of Terrestrial Radiation by the Earth's Atmosphere in Summer and in Winter" (*Pub. Am. Astron. Soc.*, Vol. III, p. 19, and *Science,* N. S., Vol. XL, pages 417 to 424, September 18, 1914), where it is shown that more than half of the radiation from the Earth's surface may escape directly to space in cold winter weather, but that about three-fourths of the superficial potential radiation are retained by the air in temperate latitudes in summer, and even more than this in the tropics. The vicissitudes of this radiation, entrapped by the atmosphere, can be partially followed, or surmised, by the modified "thermodynamics" for which we are indebted to Professor Bigelow, though not all of his conclusions will stand the test of critical examination. A correction of a few of these misleading results, which is attempted here, is very necessary to further progress.

Though he has tried to avoid the insidious errors to which this subject is exceedingly liable, the author does not assert that latent errors may not exist in the present contribution, and will welcome courteous and unprejudiced criticism.

PART I.

ATMOSPHERIC THERMODYNAMIC THEORY AND ITS LIMITATIONS

INTRODUCTION

IN approaching this subject, the question immediately arises: Can the science of thermodynamics which, as applied to the steam engine and other mechanical and physical problems, has grown up under the rigidly controlled and extremely artificial conditions of laboratory practice, and which has assumed an exact mathematical form, retain its exactness when extended to cover the conditions which are met in the free air? The purpose of the present paper is to examine some relations between diverse meteorological data, predicted in Bigelow's thermodynamic equations.* The conclusions of thermodynamic theory have also been supplemented in another direction by a consideration of Sir W. N. Shaw's *"Principia Atmospherica."* The thermodynamic principles have been treated more elaborately by Professor Bigelow in his recent book, "A Meteorological Treatise on the Circulation and Radiation of the Earth and of the Sun",† but without essential modification of the method.

Like pretty much all of thermodynamics, the derivation requires a series of algebraic transformations. We can follow and assent to the algebra; but a physical conception of the atmospheric transformations which are represented in these mathematical symbols is not so easy, and we are plunged deeper and deeper into the abstract, until our belief in scientific verities comes pretty close to a "faith in things unseen." Before the question: Can the conquests of laboratory thermodynamics be applied to atmospheric problems? can be answered fully, these problems must be visualized in all their complexity. This being practically impossible at a single glance, we must consider different elements of the problem separately.

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* Frank H. Bigelow. " The Thermodynamics of the Earth's Non-adiabatic Atmosphere." *American Journal of Science,* Ser. 4, Vol. XXXIV, pp. 515–532, December, 1912.

† New York. Published by John Wiley and Sons, 1915.

The method employed by Bigelow applies the well-recognized equations of thermodynamics to the actual conditions of the atmosphere, starting with Clapeyron's equation for pressure,

$$(1) \qquad P = \varrho R T,$$

and giving the gas-coefficient, R, whatever values may be necessary to connect the observed absolute temperatures, T, with the densities, ϱ, recognizing that the actual conditions in the free air are usually not adiabatic.

In laboratory experiments where a definite volume of air can be confined within rigid and nonconducting walls, and the changes of temperature are made at the expense of the molecular kinetic energy already included within the mass, a variation of one of the other quantities, the pressure, for example, takes place under strictly adiabatic conditions, and R is a constant. The specific heat at constant volume, which is itself only a ratio, as is also the coefficient R, in this case remains constant also. But in the free air there are no rigid walls and heat is imported from without into any given volume in varying ways, so that non-adiabatic conditions are the rule. To express these relations for the free air, the equations which have been worked out from laboratory experiments are allowed to stand; but the effective gas-coefficient, R, becomes a variable, and the specific heat of the free air at constant pressure, C_p, also becomes an effective specific heat and a variable quantity; that is to say, under the conditions in the free air, the specific heat at constant pressure is a fictitious quantity, embracing the thermal capacity of the air, together with whatever variations of condition have been imposed upon the air mass by circulation, radiation, and other processes, which abrogate the fundamental assumptions of laboratory practice; or perhaps it would be better to put it the other way, and to say that the artificial conditions of laboratory practice give a fictitious apparent constancy to ratios and coefficients which, in a more general treatment of the subject, must be considered as variables.

To apply these principles practically, the atmosphere is divided into successive layers of 1000 meters depth; and in each layer it is assumed that the downward acceleration of gravity between top and bottom must be equal to the upward component of the expansile force, or pressure of the air, as modified by whatever variations of the molecular kinetic energy of the mass may have been superinduced by agencies external to the layer.

The equation of equilibrium, as adopted by Bigelow, assumes the form (*op. cit.*, equation (22), p.518):

$$(2)\quad g\,(z_1 - z_0) = -\frac{P_1 - P_0}{\rho_{10}} - \tfrac{1}{2}\,(q_1^2 - q_0^2) - (Q_1 - Q_0),$$

in which the subscript 1 indicates the top, and subscript 0 the bottom of the 1000-m. layer, $g =$ the acceleration of gravity, z is the height, P the pressure in units of force, ρ the density (mass per unit of volume), q the wind-velocity, and $Q =$ the imported heat affecting each unit of mass in the layer. There is no term to express the heat developed by condensation of moisture, as distinct from the heat produced by absorption of radiation, an omission which is occasionally of considerable importance; and the neglect of variations in the latent heat must affect the result slightly; and these omissions may produce more or less discrepancy.

The course followed by Bigelow has been to adjust the quantities entering into the complex system of thermodynamic equations so as to produce a consistent result as applied to the lower part of the atmosphere, so far as this can be inferred from single independent sections, representing individual balloon ascensions, where the data are only moderately synchronous; but the omission of any consideration of the varying composition of the atmosphere and of the changes in its latent heat must make such a system diverge from the truth in the higher levels. I shall show by examples that enough discrepancies remain to prevent the acceptance of the scheme, as it stands, as a complete solution of the atmospheric problem, although it presents a notable advance in the right direction, especially in respect to its explanation of the prevailing non-adiabatic condition of the larger part of the atmosphere, which has heretofore been accepted as a fact, without considering its meaning.

THERMAL EQUILIBRIUM OF THE ATMOSPHERE

PRESSURE AND ITS THERMODYNAMIC EXPRESSION

WE can see clearly that the downward pull of gravity, represented by the first member of (2), must be equal to the upward component of the expansile force of the gaseous atmosphere, or else the atmosphere would collapse. In a general way, also, we can assent to the proposition that the deviation from the theoretical adiabatic temperature-gradient in the atmosphere must be due to circulation and radiation, at least in part. But is this all, and is even this statement of the case correct as far as it goes? Let us examine the meaning of the terms a little more closely.

The first member of (2), $g\ (z_1 - z_0)$, for $z_1 - z_0 = 1000$ m., is practically a constant at all altitudes above a given location which need be considered in this connection, since the altitude term of g is very small. The equation asserts that if there were no circulation and no radiation, the ratio of the difference of pressure at top and bottom of each layer to the mean density of the layer would also be constant, in which case the thermal gradient in altitude would be adiabatic at all heights. This takes no account of the change in the composition of the atmosphere as the altitude increases. From the spectra of meteors which pass through the atmospheric layers and are thereby heated to incandescence usually somewhere between altitudes 120 and 50 km., we know that the atmosphere is mainly composed of hydrogen at these heights; and here consequently, the ratio $(P_1 - P_0)/\varrho_{10}$ bears no relation to the same ratio for the lower atmosphere. The equation therefore breaks down when applied to these elevated regions, and when interpreted literally, it underrates the height of the atmosphere.

Further difficulties and apparent discrepancies, which, however, are capable of explanation, appear when the pressure term is analyzed. The sum of the pressure differences, $\Sigma \Delta P$, is of course equal to P_0, the surface pressure (if there is static equilibrium) and would be equal to P_0 no matter what distribution of pressure in altitude might be assumed. If, however, we sum the densities computed from the temperatures, and assume that the pressure is solely due to the gravitation of the successive layers, the result is evidently

$$\Sigma (\varrho_{10} \times 1000) = \varrho_0 \, l_0, \tag{3}$$

where ϱ_0 is the density of air at sea-level for normal pressure and temperature, and l_0 is the height of the homogeneous atmosphere. Taking mean European values of ϱ_{10} to the height of 18,000 m. from Bigelow's Table 2 (*Op. cit.*, p. 525) representing five balloon ascensions in latitude + 52° and one in latitude + 45°, we have these figures:

TABLE 1

Δz	$\varrho_{10} \times 1000$	Δz	$\varrho_{10} \times 1000$
0 to 1 km.	1166.3	10 to 11 km.	453.2
1 " 2	1076.1	11 " 12	405.7
2 " 3	986.4	12 " 13	362.6
3 " 4	902.5	13 " 14	325.4
4 " 5	824.2	14 " 15	292.5
5 " 6	751.4	15 " 16	262.0
6 " 7	683.3	16 " 17	234.8
7 " 8	619.7	17 " 18	208.4
8 " 9	560.2	Sum =	10,619.4
9 " 10	504.7		

The sum, $\Sigma (\varrho_{10} \times 1{,}000) = 10{,}619.4$ for a height of 18 km., is already greater than $\varrho_0 \, l_0$* reduced to the observed pressure (100,419 meter—kg.—sec.), or

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*$l_0 = 7991$ m. for air at 0° Cent. and 760 mm. pressure, $\varrho_0 = 1.29305$ kg.

$$10{,}332.8 \times \frac{100{,}419}{101{,}323.5} = 10{,}240.6;$$

that is to say, a density-law which would be accurately fulfilled to the outer limit of the air, if the atmosphere were of uniform composition and adiabatic gradient at first sight seems to fail under actual non-adiabatic conditions. Since wind-velocities increase upwards through the lower half or two-thirds of the atmospheric mass, and since on account of air friction this differential velocity exerts a lifting force at every level, though a very small one, the excess of pressure indicated by static theory is partly compensated by this lifting effect, which, however, rapidly becomes inoperative in the isothermal layer where very moderate wind velocities prevail.

Irregular departures from theory may be real variations which are averaged in the interchange between neighboring columns. In a balloon ascension, veins of air moving with various velocities and in different directions are often met at diverse altitudes, and these air currents are sometimes advertised in advance by the visible movements of their cloud contents, but would be unknown in the absence of clouds if it were not for balloon-records. These varying horizontal movements regulate the excess or deficiency of supply in neighboring air masses. In this way minor local variations of atmospheric energy are handed on to other regions and are eventually stabilized; but to get the complete balance, it is not enough to sum the quantities in a single narrow column (Eq. 3). In addition, there must be included numerous horizontal cross-sections at various levels in order to secure the whole story of the mechanism of adjustment. Not much is known at present about these cross-sections, and therefore no single vertical section of the atmosphere, taken by itself, can do more than give an imperfect picture of the conditions of thermodynamic, or of hydrodynamic equilibrium, while extrapolations beyond the limits of observation are only possible vaguely. Thus (3) asserts that the atmosphere ends at about 16 km., and while (2) may give a higher limit (somewhere between 50 and 90 km.), this also is entirely inadequate. As Bjerknes has shown,[*] if the atmosphere were homogeneous, its height would be 7835 dynamic meters (or "leometers", to use Shaw's term) = 8005 meters (for temperatures varying as they actually do between 0° C. at the surface and absolute zero at an outer limit, allowing also for the upward diminution of gravity). A gradient of 5° per 1000 dynamic meters (4°.897 per 1000 m.) would mean a height of 54,600 dynamic meters, or 56,205 meters. An isothermic atmosphere would extend to infinity. In the actual atmosphere some portions belong to the third category, while an extensive upper layer nearly fulfils the fourth condition. We may at least be sure that the height is several times 50,000 meters.

The preceding values of the density rest on the thermograph-record. A portion of the discrepancy we are considering may be due to errors of observation and to the inadequacy of the instrumental records, for the following reasons: (*a*) Owing to the rapidity of the ascent, the thermograph lags somewhat, and values of pressure and density, computed from the temperatures by Bigelow's formulas, namely,

$$P/P_0 = (T/T_0)^{\frac{n\,k}{k-1}} \text{ (\textit{op. cit.}, eq. 6),}$$

and

$$\varrho/\varrho_0 = (T/T_0)^{\frac{n}{k-1}} \text{ (\textit{op. cit.}, eq. 8).}$$

(where P, T, ϱ, are pressure, absolute temperature and density, respectively, n is the ratio of the adiabatic thermal gradient to the actual one, and k is the ratio of specific heat at constant pressure, divided by specific heat at constant volume for air) probably correspond to the true values at slightly lower levels, that is, the computed pressures will be a little too large.

(*b*) The rapid ascent makes the confirmatory pressures obtained by the aneroid too high, and they are found to be a little higher than the values computed from the thermograph-record, sometimes to the extent of 1 or 2 mm. in the European records cited,[†] where the highest altitudes reached 17 or 18 km., and presumably by larger amounts in those still higher ascents where even larger upward velocities are experienced. Since the balloon ascends very rapidly and bursts when it reaches its greatest altitude, accurate records can seldom be registered for the more or less disturbed descent from which a mean of ascending and descending values might otherwise be obtained, though the use of multiple clustered balloons, of which one or more burst before the rest, may possibly overcome the difficulty hereafter.

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[*] Dynamic Meteorology and Hydrography, p. 57.

[†] Compare F. H. Bigelow's "Meteorological Treatise" on Atmospheric Circulation and Radiation, Table 15, p. 56.

The aneroid readings, which are systematically too high, correspond to altitudes (z) which are estimated too low during a considerable part of the trip. As an example, I will take the heights and barometric pressures obtained by the observers of the U. S. Weather Bureau for the remarkable ascent at Huron, S. D., on September 1, 1910, which reached 30.5 km.* Here the ascensional velocity reached quite high values, being at times about 7 meters per second, and the lag of the instrumental registry may have been quite appreciable, though I am unable to assign definite values. The elasticity of the thin metal, of which the aneroid is made, changes when subjected to large and sudden variations of strain and to a greater extent the more suddenly the deforming stresses are applied. The amount of error will vary with different instruments. Let us assume a lag of 6 seconds in the aneroid record. If it should become possible to determine the time of this lag accurately, the computed corrections can be reduced accordingly. The seventh column in Table 2 contains the correction to the aneroid from a smooth curve, and column 8 gives the approximate heights, z', which would have to be accepted if the lag is as assumed.

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* William R. Blair. "Free Air Data. Sounding Balloon Ascensions at Indianapolis, Omaha, and Huron." Bulletin of the Mount Weather Observatory, Vol. IV, Part 4, p. 234, 1912.

TABLE 2

ILLUSTRATION OF THE ERROR IN THE COMPUTED HEIGHT FOR A HYPOTHETICAL ANEROID TIME-LAG OF SIX SECONDS

Barometer B	1st height z	Velocity $\Delta z/\Delta t$	Interval Δt	ΔB	Lag $\frac{\Delta B}{\Delta t}$	Correction to B	Corrected z'
mm.	m.	m. p. s.	min.	mm.	mm.	mm.	m.
657.6	1 234	3.37	6.1	67.9	11.1	−1.1	1 249
601.4	1 984	2.67	4.7	56.2	12.0	−1.1	2 001
526.6	3 098	2.95	6.3	74.8	11.9	−1.2	3 118
460.0	4 214	3.20	5.8	66.6	11.5	−1.2	4 238
392.6	5 474	4.03	5.2	67.4	13.0	−1.2	5 497
378.4	5 758	4.30	1.1	14.2	12.9	−1.3	5 786
341.5	6 538	6.85	1.9	36.9	19.4	−1.8	6 565
296.6	7 596	5.18	3.4	44.9	13.2	−1.5	7 621
256.0	8 665	5.08	3.5	40.6	11.6	−1.4	8 702
223.6	9 627	4.87	3.3	32.4	9.8	−1.2	9 670
169.8	11 508	6.67	4.7	53.8	11.4	−1.1	11 557
144.9	12 542	5.75	3.0	24.9	8.3	−0.9	12 588
119.8	13 789	5.33	3.9	25.1	6.4	−0.7	13 860
96.8	15 182	4.73	4.9	23.0	4.7	−0.5	15 264
71.7	17 127	6.12	5.3	25.1	4.7	−0.4	17 204
43.4	20 422	7.42	7.4	28.3	3.8	−0.3	20 470
34.4	22 000	4.38	6.0	9.0	1.5	−0.2	22 050
23.9	24 444	8.15	5.0	10.5	2.1	−0.1	24 460
16.6	26 892	6.08	6.7	7.3	1.1	0.0	26 900
9.8	30 486	6.12	9.8	6.8	0.7	0.0	30 500

It will be seen that the most rapid change of pressure, and therefore the most severe strain on the aneroid, occurred when the balloon was passing the altitude of 6500 m., where, according to the supposition on which the table is founded, the aneroid-lag may have been as much as 1.8mm. for a short time, or proportionately less if the lag in time was shorter than 6 seconds. Above 20 km., the aneroid-lag, according to the hypothesis, was less than 0.2 mm., and it became insensible at 30 km. in case the mechanism was trustworthy in every other respect. The assigned altitudes (z) appear to have been computed from the theoretical ascensional force of the balloon, and not from the aneroid readings. The latter, when reduced by the ordinary barometric formula, give slightly different values, such as 5739 m. instead of 5758 m., 11468 m. instead of 11508 m., 20533 m. instead of 20422 m., etc. The influence of the lag on the corrected heights (z') is the greatest at about 16 km. Apparently the assumed lag of 6 seconds is about right.

The above illustration lays the burden of error on the aneroid; but a part of the total discrepancy must be attributed to errors in the thermograph whose lag makes the temperatures, and thence the densities computed by the thermodynamic theory, too large, just as the aneroid-lag makes the directly observed pressures too large. But these instrumental errors are not the sole, or the main cause of discrepancy. The thermograph errors are smaller than those of the aneroid, and the latter are far too small to meet the issue raised in the discussion of Table 1. Moreover, even if we had entirely correct values of P and ρ, the ratio, $(P_1 - P_0)/\rho_{10}$, would not be constant at different altitudes, because adiabatic conditions almost never exist in the atmosphere, and the last two terms of (2) have a considerable and varying magnitude. In the isothermal layer is a large body of air in which temperature inversion is perpetual. Consequently, since this layer is warmer and rarer than perfect adiabatic stability permits, it has an ascensional potential which diminishes the full downward pressure appropriate to its density, and the overlying layers of much colder air have a descending potential which has the effect of increased density. Even if other factors prevent convectional overturning, the potential must be there. To a smaller extent, the same is true for any layer which deviates even a little from adiabatic conditions. The thermodynamic equation says that the lower layers are too dense; but the hydrodynamic principle says that, nevertheless, they must have an ascensional potential which counteracts their extra weight. The dilemma requires that these rival principles, which have heretofore employed diverse or opposing formulas, shall be reconciled, and that both shall be applied.

The temperature- and pressure-curves in an altitude plotting are not smooth curves of regularly varying curvature, but have sudden small variations. The ordinary deviations of the temperature-curve up to altitude 35 km. are now pretty well known; but observations are not accurate enough to give the pressure-curve with minute precision, and unless simultaneous theodolite observations from two ends of a base-line are perfected, there can be no independent determination of altitudes at great heights, except those derived from an imperfect theory which neglects outside interference. Professor Robert W. Willson has shown* that it is possible to determine the height of an aeroplane with considerable precision by means of ordinary sextants. With theodolites of sufficient power, and automatic records of the pointings in altitude and azimuth, there should be no difficulty in getting accurate heights for free balloons.

THE MEANING OF THE CIRCULATION TERM AND THE FURTHER NECESSITY FOR A BALANCING THERMAL TERM IN THE GENERAL EQUATION

The circulation term of (2), namely, $-\frac{1}{2}(q_1^2-q_0^2)$; where the mass-factor does not appear because we have divided by ρ, represents the component of an effective horizontal vortex in the 1000-m. layer, which tends to restore any layer of temperature-inversion to a condition of stability; but in the isothermal layer, the horizontal air-movement is so small that this process is ineffectual, and the great burden of maintaining a balance is thrown upon the thermal mechanism of molecular kinetics and radiation. These are combined in the final term of (2) $-(Q_1-Q_0)$,† which assumes something of the nature of a residual, or a quantity which is needed to balance the equation, and one which is open to much uncertainty, because any errors resulting from inaccurate determination of the other quantities remain disguised. By an adjustment of the intricate mesh of computations, the errors may be distributed and fairly consistent results may be obtained for the atmosphere as a whole; but such results can hardly be other than approximate and provisional. If they lead to conclusions which can not be independently verified, they should be received with caution.

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* *Proc. Am. Acad. of Arts and Sciences*, Vol. XLVII, p. 27, May, 1911.

† By equations (103) and (224) of Bigelow's "Treatise," $Q=J+H+W$, where J is "the inner potential energy of molecules and atoms" $=J_m$ (or "inner molecular potential energy") *plus* J_a (or "inner atomic potential energy") by eq. (99), H is "the inner kinetic energy of molecules and atoms," and W is "the work of total external potential energy." The sum of $H+J$ has also been denoted by the symbol U. But by eq. (118) $\frac{J_a}{Q}=1-\frac{J_m}{Q}=1-\frac{3}{2}\frac{Cp-Cv}{Cv}=\frac{5\,Cv-3\,Cp}{2\,Cv}$. Here the specific heats at constant volume (Cv) and constant pressure (Cp) have been inserted, but instead of J/Q, unity has been written in the first terms of the second and third members, so that the equation is wholly erroneous. Thus by (99) and (118), $J=J_m+J_a=J_m+[Q-J_m]=Q$,

The discrepancy noted in connection with Table 1 requires the application of an upward acceleration, or its equivalent, for its removal, and several partial remedies have been suggested. Thus a small portion of the discrepancy is no doubt due to lag in the aneroid reading. The delicacy of the thermograph seems to be very nearly great enough to take care of the gradual change of temperature. Besides these, the lifting effect of a strong upper current has been mentioned.

The subject of aerial friction is inadequately treated in a single paragraph of Bigelow's "Treatise" (p. 179). It is known that the velocity of an air current enters into the equations for air friction with a variable exponent, according to the greater or smaller development of turbulence, and that small-scale operations generally require the square of the velocity. Though a surface phenomenon, taking place in the free air between definite strata having different velocities, the almost indefinite multiplication of such surfaces in the transitions of the velocity-gradient in a vertical direction makes the friction in this case virtually a

which disagrees with the fundamental equation (103) = (224), and also with a further definition on p. 26: "Q = the heat or total inner energy = CvT." Evidently, by (103) = (224), Q represents the total *thermal* energy, internal as well as external, or kinetic *plus* potential energy. When we speak of inner thermal energy, reference is made to such energy as becomes manifest in ordinary thermal operations. The immensely greater inner energy of atomic formation cannot be touched and is only slightly exhibited in radio-active transformations. It will make the subject clear if we say that the free heat in a given layer, $Q_1 - Q_0$, is the excess of the inner thermal energy ($U_1 - U_0$) above that needed to perform the work of expansion ($W_1 - W_0$). The change of entropy in the layer is $S_1 - S_0 = (Q_1 - Q_0)/T$. In the case of an adiabatic atmosphere, $Q_1 - Q_0 = 0$, there is perfect equality in the radiant exchange between the neighboring air masses, and no change of entropy occurs.

volume effect. The small-scale turbulence everywhere present in moving air may be recognized in the perpetual minute fluctuations of the wind which are recorded by a delicate anemograph. On account of the absence of a generally accepted theory, I have preferred to base my estimates on experiments. I found that the velocity of fall of a horizontal plane, 1 foot square and weighing 1 pound, was reduced to about one-third of the value which it had in calm air by a horizontal wind of approximately the velocity recorded at a height of 5 km. If the extensive layer between this height and the surface constitutes a single current, then, assuming that the diminution of wind velocity between the upper level and the earth's surface is solely due to friction, and that the total force is the same, whether the air friction is immediately and directly applied to a solid surface, or is exerted upon the distant surface of the ground through successive changes in an intervening layer of air, the corresponding lifting force on an air column of 1 sq. foot section would be about 2/3 of a pound. But this is only about 1/3000 of the total air pressure and is quite negligible. From the ground (altitude = 526 m.) up to 5000 m., the mean annual wind-velocity, according to Blair's summary for five years at Mount Weather, increases from 3.5 m./sec. to 21.6 m./sec. But not all of this change (that is, the lessening of the wind near the ground) can be a frictional effect, since the air pressure diminished upward in the ratio 1 : 0.58, and as velocity with uniform momentum would have to increase in proportion to diminution of density, the lifting effect of the upper air current can hardly be more than 1/5000 of the total pressure.

In addition to the inefficiency of the frictional cause by virtue of its part in the lifting effect (we are not now considering its office in generating heat by destruction of momentum which is a wholly different and much greater effect) we have the further fact that notable variations both of direction and velocity occur in the wind at certain levels, because the wind is part of a mechanism of adjustment of differences of pressure between neighboring air-masses in a horizontal direction. Hence a frictional law applying to vertical pressure relief would be confined to the lowest of these levels.

But though friction is ineffectual to produce upward motion in the way we have been considering, it has an important part to play in an entirely different sense in regard to the maintenance of an unstable equilibrium in the lower atmosphere. Here is a large body of air whose temperature exceeds the normal value for perfect equilibrium. To attain equilibrium, this warmer air should rise and be replaced by colder air from a higher level. Consequently, the lower air is affected by an upward resultant gravitational potential which overcomes and compensates the downward acceleration of gravity upon a portion of its mass. But the overturning does not take place, because the unbalanced bodies of air are far apart, and their motion toward each other is prevented by aerial viscosity. The tendency of the lower uncompensated masses to rise may have a little influence on the differential circulation of the wind since ascending velocities prevail in the wind circulation below the isothermal layer. Evidently, the correlative descending motions must occur at times of little wind, as in calms at the center of anticyclones. The vertical component of the circulation term, $-\frac{1}{2}(q_1^2 - q_0^2)$, derived from the wind velocity, q, opposes gravity as a rule in the region below the isothermal layer, while the reverse holds above this limit.

The final result is that neither of the causes enumerated is competent to provide for the relief of the excess of pressure and density in the lower layers of air to more than a trifling extent; but since air pressure is very largely maintained by the intermolecular exchanges of radiation, so that *the curve of density agrees almost precisely with that of the variation of the radiant potential of the atmosphere in a vertical direction,** it is evident that the excess of pressure in the lower atmosphere (so far as it is real, that is, not an appearance due to instrumental error) is wholly, or almost wholly, produced through heat introduced by absorption of radiation, and that the tacit assumption of Table I, that gravitation alone is responsible for pressure of air at a given level, is not correct, although, since thermal accessions must equal losses in the long run, the gravitational law of equivalence between density and pressure is fulfilled for the entire atmosphere in a general average.

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* See my paper "On the Solar Constant," *Am. Jour. Sci.*, Vol. XXXIX, p. 206-207, February, 1915. For a further consideration of the subject, see below (p. 55).

THE MEANING OF THE HEAT-TERM IN EQUATION (2)

Considering equation (2) dimensionally, the first term,

$$g(z_1 - z_0) = \text{acceleration} \times \text{height} = \left\{\frac{L}{T^2} \times L\right\} = \left\{\frac{L^2}{T^2}\right\}$$

The second term,

$$\frac{P_1 - P_0}{\varrho_{10}} = \text{pressure} \div \text{density} = \left\{\frac{M}{LT^2} \Big/ \frac{M}{L^3}\right\} = \left\{\frac{L^2}{T^2}\right\}$$

The third term, $\frac{1}{2}(q_1^2 - q_0^2) = \text{square of velocity} = \left\{\frac{L^2}{T^2}\right\}$

The fourth term involves Q which relates to thermal energy imported from without. If $Q_1 - Q_0$ represent the transference of thermal energy without any restriction, its dimension will be that of energy, $[ML^2/T^2]$. In order to harmonize with the other terms of the equation, $Q_1 - Q_0$ must represent a quantity of thermal energy *per unit mass,* that is, the dimensional equation requires that M shall be taken out, so that the last term shall have the same dimension as the others.

An example from the mean results for the European ascensions will show the meaning of the terms and their relation. The differential terms relate to the layer immediately below the given altitude. Take the height, $z = 10{,}000$ m., for which $T = 226°.0$ *Abs. C.* In the next 1000 meters below this altitude, the temperature gradient is $\Delta T = -6°.8$. Since the adiabatic gradient is $-9°.87$ per 1000 m., there has been supplied in the layer between 9 and 10 km., either by absorption of radiation, or by transference of warm air by convection or circulation from neighboring warmer masses, or by molecular penetration from such masses, an amount of thermal energy sufficient to raise the air temperature $3°.07$ above the temperature which it would have if no heat were allowed to enter or escape from the layer. This, apportioned equally throughout the entire vertical column of 1000 m. height and 1 sq. cm. section, gives

$\frac{1}{2} \times 3.07 = 1^\circ.535$ accession of temperature to each cubic meter of air of mean density,

$$\varrho_{10} = 0.5048 \text{ kilogram per cubic meter;}$$

or taking the specific heat of air $= 0.238$, each cubic meter of air receives from without,

$$1.535 \times 0.5048 \times 0.238 = 0.1844 \text{ large cal./sec.}$$

The thermal change within the layer derived by means of (2) is $Q_1 - Q_0 = -1089$ joules/kgm. sec., that is, each kilogram of air in the layer receives 1089 joules per second, or each cubic meter receives

$$1089 \times 0.5048 \times \frac{1}{4.186 \times 10^3} = 0.1313 \text{ large cal./sec.}$$

But the wind-velocity diminished in ascending, from 20.7 m./sec. at 9 km., to 19.3 m./sec. at 10 km. Hence a portion of the heat exhibited must have come from air friction in the reduction of wind-velocity, and not all of the 0.1844 large cal./sec. came from without. The wind-variation, however, only accounts for 24.9 joules/sec., or 0.0059 large cal./sec. Thus the difference,

$$0.1844 - 0.0059 = 0.1785,$$

still remains larger by 0.0472 kgm. cal./m^3 sec. than the value assigned to $Q_1 - Q_0$ by (2).

From the thermodynamic equations, Bigelow derives four different expressions for $Q_1 - Q_0$:

(*Op. cit.*, eq. 36) $Q_1 - Q_0 = Cv_{10}(T_a - T_0) + P_{10}(v_1 - v_0)$ (36_a)

$$= T_{10}(S_1 - S_0). \qquad (36_b)$$

$$= (Cp_a - Cp_{10})\,(T_a - T_0) \qquad (36_c)$$

$$= Cp_a(T_a - T_0) - \frac{P_1 - P_0}{\varrho_{10}} \qquad (36_d)$$

Here the subscript *a* applied to temperature or specific heat, denotes the adiabatic value, *v* is the volume of unit mass, *S* is the *entropy,* and the other quantities have their usual significations. Form (36_a) can not be used here, because the volume is not constant, and the specific heat at constant volume is not appropriate. The others give the following values:

$$Q_1 - Q_0 = -1083 \qquad (36_b)$$
$$\text{,,} \quad = -1131 \qquad (36_c)$$
$$\text{,,} \quad = -1079 \qquad (36_d)$$

The mean, $Q_1 - Q_0 = 1096$ joules/sec., differs slightly from the value which I have cited above from Bigelow's Table 2 where there seems to have been a small adjustment.

Correction of Some Minor Errors

On page 532 of Bigelow's article we read: "The mechanical equivalent of heat in the *K. M. S.* system is 4185.57 kilogram meters or 4.18557×10^{10} *C. G. S.* for 1000 meters in the stratum", where the words "for 1000 meters in the stratum" are superfluous, since the mechanical equivalent of heat is not altered by the adoption of any special layer, and "kilogram meters" should read *joules.* This is evident from the attached number, as well as from the example appended, where, however, the values of $Q_1 - Q_0$ in Table 2 (*op. cit.*) are said to be in mechanical units of heat "*K. M. S.*", and they are treated as if such, and are directly converted into calories. But, as I have explained above, the tabular values of $Q_1 - Q_0$ are for unit mass and must be multiplied by the density before conversion into calories, for the calory is a unit of thermal energy referred to a definite mass of water. The direct conversion of $Q_1 - Q_0$ into calories without the introduction of the density, amounts to assuming that $\varrho = 1$. This is very nearly true for an altitude of 2 km., and thus the values obtained in this way are not very different from the truth in the lower, or convectional layers of the atmosphere; but large errors would be made if the same conversion were attempted in the upper air.

COMPUTATION OF THERMAL QUANTITIES APPERTAINING TO VARIOUS ATMOSPHERIC LEVELS

Introducing the density in the product $\varrho_{10}\ (Q_1 - Q_0)$, this may be converted into gram-calories per sq. cm. per minute by multiplying by the factor 0.001434,* since

$$1 \text{ joule/sq. m. sec.} = 10^7 \text{ ergs/sq. m. sec.}$$
$$= \frac{10^7}{4.1851 \times 10^7} \frac{\text{gram-cal.}}{\text{m.}^2 \text{ sec.}}$$
$$= \frac{10^7 \times 60}{10^4 \times 4.1851 \times 10^7} \frac{\text{gram-cal.}}{\text{cm.}^2 \text{ min.}}$$

In this way the European data for $Q_1 - Q_0$ contained in Bigelow's Table 2 in mechanical units give the same quantities in thermal units in the last column of the next Table.

TABLE 3

VARIATION OF VERTICAL THERMAL EXCHANGE AT DIFFERENT LEVELS

z	ρ_{10}	$Q_1—Q_0$	$\rho_{10}(Q_1—Q_0)$ j/m.^{2} sec.	gram. cal. /cm.^{2} min.
18 km.	0.2084	−3459	720.7	1.033
17	0.2348	−3231	758.6	1.087
16	0.2620	−2791	731.3	1.048
15	0.2925	−2392	699.7	1.003
14	0.3254	−1955	636.0	0.912
13	0.3626	−1702	617.0	0.885
12	0.4057	−1381	560.4	0.803
11	0.4531	−1190	539.2	0.773
10	0.5048	−1089	549.8	0.788
9	0.5602	−1131	633.7	0.908
8	0.6197	−1034	651.2	0.934
7	0.6833	− 920	628.7	0.901
6	0.7514	− 793	595.8	0.854
5	0.8242	− 650	535.7	0.768
4	0.9025	− 530	478.3	0.686
3	0.9864	− 380	374.9	0.537
2	1.0762	− 241	259.4	0.372
1	1.1663	− 77	89.8	0.129
			Mean =	0.801

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* A different and incorrect conversion-factor has been used by Bigelow in his "Meteorological Treatise," concerning which see my Review of his book

If this free atmospheric heat were a continuous vertical flux through a column of 1 sq. cm. section, it would represent on the average about 0.8 gram-calory of heat passing through this area

in *Science* for December 3, 1915. Since I had previously pointed out this error in my paper "On the Solar Constant" (*American Journal of Science*, Vol. XXXIX, p. 203, February, 1915), and since Professor Bigelow, while recognizing the validity of some of my criticisms (at least to the extent of substituting a "Change of Theory" in his "Meteorological Treatise," published near the end of 1915, *cf.* p. 394, in place of the erroneous one which appeared in Vol. XXXVIII, pp. 277–281 of the *Journal*, and which was the subject of my criticism), not only continues his erroneous practice in regard to the transformation-factor, but attempts to justify it at page 415 of the aforesaid "Treatise," I will enter here more minutely into particulars.

The calory is the unit of thermal energy in unit equivalent volume of water. As a unit of heat, its dimension is that of energy $[M L^2/T^2]$ and, if its transformation into radiation at unit surface is considered, may be taken as $[M/L T^2$ deg. C.$]$ But the calory is not, properly speaking, the unit of radiation, and has only been accepted as such by an elliptical use of language with the meaning that the radiation passing through the unit of area represents, or *is equivalent to*, a certain amount of thermal energy. The failure to note this fact has been productive of many misunderstandings in scientific literature, and is commented on most energetically by Chwolson in his *Traité de Physique*. In order to counteract and prevent this unfortunate ambiguity, I have proposed the use of a distinct C. G. S. unit of radiation, the "radim" (*Astrophysical Journal*, Vol. VIII, p. 271, December, 1898; also *Meteorologische Zeitschrift*, October, 1901, p. 472), but it has not been generally accepted.

Since radiation always represents transition of energy from the thermal to the radiant form *at a surface*, it is necessary that the element of surface, or of *flax through a surface* should be retained in the radiant constant. In the thermo-dynamic treatment of an air column, we do not consider the radiation of a unit cube of air through all six of its faces, but only the radiation through the single upper face in a vertical direction, because the volume is part of a layer of like temperature at the sides.

Radiation exists only in the ether, and for *its* volume expression, the kilometer which is the length considered by Bigelow in the atmospheric columns of the earth, or even 1,000 kilometers for the sun, do not constitute appropriate units for light. We need to employ the distance traveled by light in one second (velocity of light $= c$). But in simply stating the transition at a surface from the thermal to the radiant mode, it is not necessary to explicitly mention *ethereal* volume-energy, although it is of course implied in the definition of the new term, "radiation"; nor would the explanation of such a quantity be a simple matter, as will be evident from my paper on "The Luminiferous Ether," No. 2 of this series.

Whenever it is necessary to consider the *volume*-energy of the ether, it is proper to use Planck's $a = 4\sigma/c$; but σ should always be kept to represent every minute from the surface up to 18 km. in order to maintain atmospheric equilibrium and restore losses in north temperate latitudes. A portion of this heat is imported from more southerly

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radiant flax through a surface, and its dimension is *not the same* as that of a, but the dimension of a being $[M/LT^2]$ for unit area, that of σ is necessarily $[M/T^3]$ for unit volume, since the factor changing from σ to a includes $1/c=$ dimension [T/L]. Bigelow's value of σ (M.K.S.) in mechanical units, namely "$\sigma=5.1210\times10^{-18}$" ("Meteorological Treatise," foot-note to p. 280), is therefore wrong, and should be $\sigma=5.1210\times10^{-15}$; and in his attempts to justify his course he has contradicted himself repeatedly, and tangled up his argument in fallacies.

One source of Bigelow's error seems to be this: He has adopted a peculiar method, founded on inferences, in obtaining his constants, c and α, in his equation for the radiant potential of the air, $K=cT^{\alpha}$ (note that here c does not mean velocity of light, but a certain numerical factor to be determined experimentally), and he is obliged to interpret the result. If we were dealing with the surface of a solid, c would be identical with Kurlbaum's constant (σ) for surface radiation and α would be 4. But when on p. 126 of his "Meteorological Treatise," Professor Bigelow attempts to apply his equation to the air and to get radiation in terms of air-volume, he falls into amazing absurdities, and concludes that "air radiates at six times the rate of a perfect radiator"!

Now it is permissible to make the substitution of radiant air-volume for radiant solid-surface, if this is done carefully and rationally, since σ, if expressed in calories, is given as *equivalent* thermal energy contained in unit volume of water, but transmitted as *radiation* through unit area per $(\text{degree})^4$. Here, since *unit* volume and *unit* surface are predicated, we can omit either or both of these factors dimensionally, if we wish. Thus we can think of the thermal energy in a volume of matter as volume-energy which is about to be transformed into radiant flux; or we can think of the energy as surface-flux in the unit of time; or, finally, we may eliminate both volume and surface and speak only of the *mass* of unit volume of water whose temperature varies by 1 degree on account of the given flux. Dimensionally, these three interpretations are

$$\left\{\frac{ML^2}{T^2}\Big/L^3\right\}=\left\{\frac{M}{LT^2}\right\},\ \text{if area}=1,$$

$$\left\{\frac{ML^2}{T^2}\Big/L^2T\right\}=\left\{\frac{M}{T^3}\right\},\ \text{if volume}=1,$$

or simply $[M]$, if $T=1$ and both volume and area $=1$. But having adopted either of these provisional definitions, great care must be exercised in dealing with substituted equations where possibly a different standard is implied and lack of such care is a possible source of some of Bigelow's mistakes. Thus in passing from M.K.S. to C.G.S. values, where a factor, 0.000014336 would be appropriate for Planck's "a," he applies the same factor to Kurlbaum's "σ," getting results which are only one one-hundredth of the true ones.

latitudes and a part is more immediately supplied by the sun's rays. The speed of the flux would have to vary through a very wide range, and the average for the entire air-column must be

It is true that on page 417 of the "Treatise," it is said that "the equation (b), page 414, $K_{10}=c\ T^{a}$, gives c of the same dimension as a, and it must be multiplied by a velocity to become comparable with σ in the Stefan Law," where a evidently refers to Planck's "a" and not to the exponent of T, and K is obtained from equations all of whose terms have the dimension $[L^2/T^2]$, as is shown in the present paper (*ante*, p. 21); and in the same place Bigelow explicitly states that "a is the work energy per unit volume." Nevertheless, in his Table 84 (*op. cit.*, p. 378), thermal quantities which purport to be given in "gr. cal./cm^2 min.," or which are expressed as functions of the *surface*, have been obtained with the factor which applies to "energy per unit volume."

I cannot, of course, tell just how Professor Bigelow came to make his mistake, but it seems possible that he has replaced c by σ in his equation for K in spite of his own warning (*op. cit.*, p. 417).

I note that Bigelow gives density (M.K.S.) as kilograms per *cubic meter* (Table 95, *op. cit.*, p. 408), and the mechanical equivalent of heat (A) as joules per large calory. But the large calory is the thermal equivalent of 1 degree, not in a *cubic meter*, but in a cubic *decimeter* of water; and since the cubic meter contains 1000 cubic decimeters, in transforming σ derived from the equation for K in the M.K.S. system into a σ which can be divided by A in the D. K. S. (decimeter, kilogram, second) system, the M. K. S. value of σ must be divided by 1000, and the factor ($\times 10$) reducing from joules / m.^{3} to ergs / cm.^{3} becomes $\frac{10}{1000}=\frac{1}{100}$. The division by 1000 has not been explicitly performed by Bigelow in this case, and this is possibly the source of the discrepancy. The values of σ in mechanical units (M. K. S), unless derived from volume-energy in D. K. S. units, will be 100 times too large if a D. K. S. value of A has been used in the transformation between mechanical and heat units. The exponent of the factor 10^{-6} in the M. K. S. value of σ (*op. cit.*, p. 126) should be -8, instead of -6, that is, the sentence should read: "The Kurlbaum coefficient in the Stefan formula for a perfect radiator is taken at 7.68×10^{-11} (C. G. Min. C°) $=5.32\times 10^{-8}$ joules per square meter per sec."; and the elimination of this error destroys the ground for the extraordinary announcement on the same page that air radiates better than a perfect radiator. This error being eliminated, we can then take the correct dimension of σ, or $[M/T^3\ (\mathrm{deg})^4]$ and can take D. K. S. units of volume-energy as equivalent to the customary M. K. S. units of radiant flux, and transform M. K. S. units of radiant flux directly to gram-calories per cm.^{2} per min. by the correct reduction factor ($\times 0.0014336$). The figures will "check" as before, but what is more to the point, they will have been obtained by a computation which is correct at every step without any illusion as to the position of the decimal point; and the misleading conversion-factor being now cast out, the whole series of fantastic and erroneous conclusions which have been founded upon it go with it.

quite different, as may be seen from the following considerations: The apparent flux of heat lags greatly in certain layers and therefore can not be directly compared with a flux of radiation which has a constant velocity. Still, as the heat is largely derived from absorption of radiation, some connection between the two ought to be evident. This apparent thermal flux (which sometimes represents heat transferred from other bodies of air, and sometimes is heat supplied *in situ* by absorption of radiation from outside sources, either terrestrial, or solar) is going on throughout the entire atmosphere, but with variations in the mode at different levels. Storage of heat in certain layers is so important a part of the process that the above figures can not be directly compared with the influx of solar radiation. If the solar constant is assumed to be 3.6 small calories per sq. cm. per min., this distributed over four great circles of the sphere would give an average return radiation of 0.9 cal./cm.^{2} min., if it were not for the very large losses by reflection back to space. This reflection cannot possibly enter into the thermal equation, and therefore the true average return of an atmospheric radiation, equivalent to that directly received from the sun, could not be as large as 0.8 cal./cm.^{2} min. unless the solar constant is greater than 3.6; and hence the rate through the atmosphere as a whole must be considerably smaller than 0.8, while the exceptionally large thermal rates found in some layers imply local accumulation of heat, either imported through the atmospheric circulation, or obtained by exceptional local absorption of radiation.[*] In fact, neither the average, nor the sum of these thermal quantities can be equated to the solar radiation received in one minute, though, of the two, the average comes nearest to such a relation. In equating the sum to the total flux, Bigelow has committed an astounding anachronism. The time-factors are totally diverse. The actual heat in the lower layers of the atmosphere at a given time represents the storage of energy from many hours of sunshine, accumulated during an interval which covers some weeks. In the warmer half of the year, the gain of thermal energy exceeds the loss and the temperature-curve rises. The maximum temperature at the earth's surface occurs 6 or 7 weeks after the sun has reached its greatest altitude and this lag gives a measure of the slowness with which the atmospheric processes act, which are concerned in the storage of heat.*

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[*] The mean discrepancy would be less notable if the average were to be carried to higher levels, where the density is small and very little of the inner energy ($U_1 - U_0$) is needed to perform the work of expansion ($W_1 - W_0$). Here $Q_1 - Q_0$ increases, but the product ρ_{10} ($Q_1 - Q_0$) diminishes.

The very notable increase in the thermal flux in the isothermal layer which is shown in Table 3, and concerning which Bigelow estimates that the average rate derived from his thermodynamic computations is approximately doubled, shows that there must be great thermal congestion in this layer; and this, in the writer's opinion, confirms the suggestion made in his treatise on "Atmospheric Radiation"† (p. 123) that air at 20,000 meters is probably much more heated by the sun's rays than air at 1000 m., because the upper layers are the first to attack the unsifted radiation. It is to this powerful incipient absorption that we must attribute the existence of the isothermal layer.‡

AN INDEPENDENT COMPUTATION OF THERMAL QUANTITIES AT VARIOUS LEVELS

In Table 4, using the same series of European values, I have obtained the imported heat, ΔQ, from the mean departure of the vertical temperature-gradient,

$$\tfrac{1}{2}\{\Delta T - (-9^{\circ}.87)\},$$

where ΔT is the actual temperature-gradient, and $\Delta T - (-9^{\circ}.87)$ is the departure from the adiabatic gradient, using the formula,

$$(4) \qquad \Delta Q = \tfrac{1}{2}\{\Delta T - (-9^{\circ}.87)\}\,\varrho_{10} \times 0.238,$$

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* See F. W. Very, "On the Solar Constant." *Am. J. Sci.*, Vol. XXXIX, p. 201, February, 1915.

† The writer's suggestion in *Weather Bureau Bulletin G.* (1900), was founded on his observations of air radiation (1892–1895), which gave results extended to the upper layers of the air through rational considerations, and not by direct observations at great altitudes; but the conclusion has been confirmed by the sounding-balloon results.

‡ See F. W. Very, "Sky Radiation and the Isothermal Layer," *Am. Jour. of Sci.*, Ser. 4, Vol. XXXV, p. 369, April, 1913.

ϱ_{10} being the weight of a cubic meter of air of the average density of the layer, and 0.238 the specific heat of air referred to water. These values are given in kilogram-calories per sq. meter per second, and for comparison there are annexed the corresponding values of $Q_1 - Q_0$ or heat change per unit mass from Bigelow's Table 2, which, being multiplied by ϱ_{10}, are converted into kgm. cal./m.² sec. by dividing by 4186. The latter follow from the equation of the thermodynamic theory, but the former simply rest on observed departures from an adiabatic rate of temperature change without an elaborate theory.

TABLE 4

THERMAL ACCESSIONS (ΔQ) AT DIFFERENT LEVELS, ASSUMING THAT ADIABATIC DEPARTURE DEPENDS ON HEAT IMPORTATION

ΔT	Departure ΔT —(—9°.87)	Mean Departure	ΔQ kgm. cal. /m.² sec.	$\varrho_{10}(Q_1 - Q_0)$ kgm. cal. /m.² sec.	Height z
+5.°1	+14.°97	7.°485	0.3713	0.1722	18 km.
−3.9	+5.97	2.985	0.1668	0.1812	17
+2.2	+12.07	6.035	0.3763	0.1747	16
+0.9	+10.77	5.385	0.3749	0.1671	15
+4.6	+14.47	7.235	0.5603	0.1519	14
−0.1	+9.77	4.885	0.4215	0.1474	13
−2.9	+6.97	3.485	0.3374	0.1339	12
−7.3	+2.57	1.285	0.1386	0.1288	11
−6.8	+3.07	1.535	0.1844	0.1313	10
−8.3	+1.57	0.785	0.1047	0.1514	9
−7.9	+1.97	0.985	0.1453	0.1531	8
−7.1	+2.77	1.335	0.2171	0.1502	7
−6.5	+3.37	1.685	0.3013	0.1423	6
−5.7	+4.17	2.085	0.4091	0.1279	5
−6.0	+3.87	1.935	0.4157	0.1142	4
−4.8	+5.07	2.535	0.5955	0.0895	3
−5.7	+4.17	2.085	0.5346	0.0620	2
−4.6	+5.27	2.635	0.7319	0.0215	1

There is a wide discrepancy between the results of columns 4 and 5 in Table 4; but it is not easy to see why the simpler method is not valid. The large values of ΔQ near the surface seem to signify that here the deviation from the adiabatic law is due to extraneous importation not considered in the thermodynamic equations. The lower layers of air from the surface up to 7 km. absorb terrestrial radiation, especially near the surface where aqueous vapor is more abundant; and in this way, with possibly some thermal additions from condensation of moisture, this part of the atmosphere receives an exceptional accession of heat. This heat furnishes the motive power for the mechanism of convection. The values of ΔQ are in excellent agreement with what we know to be transpiring in these layers near the ground. In the next four kilometers there is but little absorption and the deviation from the adiabatic gradient is smaller; but on entering the isothermal layer, the adiabatic departure becomes again exceptionally large, and there are new accessions of heat, this time, we may believe, ~~for~~ from the absorption of solar radiation. The general and rather gradual increase of the free heat (as computed by the thermodynamic formula) in an upward direction (col. 5) is a result of the successive removal of portions of obstructing atmosphere and a freer passage of the air's own radiation outward to space, as well as the freer entrance and absorption of the sun's rays. A partial reversal of this progression just below the isothermal layer, heralds the proximity of this region where ΔT changes sign.

In Table 5, the small circulation term has not been separated from the thermal term, and the algebraic sum of the gravitational term and of the ratio of change of pressure/density, indicated by $(Q_1 - Q_0)'$, has been computed by the equation

$$(5) \quad (Q_1 - Q_0)' = \{ g(z_1 - z_0) \} - \{ (P_1 - P_0)/\varrho_{10} \}$$

The values adopted by Bigelow for $Q_1 - Q_0$ differ somewhat from these, not only on account of my omission of the circulation term, but also apparently by his omission of minor, or local variations and a general adjustment to smoothed values. The value of g used here is 9.81, and the values of $(P_1 - P_0)/\varrho_{10}$ contained in col. 4, subtracted from 9810, give the computed values of $(Q_1 - Q_0)'$ in col. 5; col. 6 contains the observed ΔQ (from Table 4) in mechanical units (joules/sq. m. sec.); and col. 7 gives their difference.

TABLE 5

COMPARISON OF THE VERTICAL DISTRIBUTION OF THERMAL ACCESSIONS, ΔQ, ON WHICH RADIATION DEPENDS, COMPUTED FROM THE ADIABATIC DEPARTURES, AND $(Q_1 - Q_0)$, COMPUTED FROM BIGELOW'S THERMODYNAMIC THEORY

z	$P_1 - P_0$*	ϱ_{10}	$\frac{P_1 - P_0}{\varrho_{10}}$	Mechanical units		
				$(Q_1 - Q_0)'$	ΔQ	$(Q_1 - Q_0)' - \Delta Q$
18 km.	1 290	0.2084	6 190	3 620	1 554	+2 066
17	1 842	0.2348	7 845	1 965	698	+1 267
16	1 836	0.2620	7 008	2 802	1 575	+1 227
15	2 153	0.2925	7 360	2 450	1 570	+ 880
14	2 045	0.3254	6 285	3 525	2 345	+1 180
13	2 925	0.3626	8 067	1 743	1 764	− 21
12	3 399	0.4057	8 377	1 433	1 412	+ 21
11	3 857	0.4531	8 513	1 297	580	+ 717
10	4 408	0.5048	8 734	1 076	772	+ 304
9	4 903	0.5602	8 752	1 058	438	+ 620
8	5 463	0.6197	8 814	996	608	+ 388
7	6 081	0.6833	8 900	910	909	+ 1
6	6 766	0.7514	9 005	805	1 261	− 456
5	7 534	0.8242	9 141	669	1 712	−1 043
4	8 369	0.9025	9 274	536	1 740	−1 204
3	9 308	0.9864	9 436	374	2 493	−2 119
2	10 315	1,0762	9 581	229	2 238	−2 009
1	10 074	1.1663	8 634	1 176	3 064	−1 888
			Sums =	26 664	26 735	

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* Pressures (P) are given in M. K. S. units of force (100,000 M. K. S. force units per sq. meter = 1,000 millibars = 1,000,000 dynes per sq. cm.) The unit of atmospheric pressure is the bar = 1,000,000 dynes pressure per sq. cm.

Comparing the values of $(Q_1 - Q_0)'$ with the observed ΔQ, we find that $(Q_1 - Q_0)'$ is the larger of the two in the isothermal layer, but is too small near the surface of the ground, as was the case with $\varrho_{10}\ (Q_1 - Q_0)$ in the preceding Table. The differences in the last column of Table 5 show the deviations from an incomplete theory and demonstrate that the thermodynamic theory *is* incomplete. Moreover, the omission of the small circulation term can not account for the variations.

The values of ΔQ in Table 4 are given in thermal units.* Those in Table 5 are in mechanical units. The computed value of $(Q_1 - Q_0)'$, multiplied by the mass of 1 kilogram, gives a quantity of thermal energy in joules/m^2 sec., with which must be compared the mean accession of thermal energy in the layer, obtained by multiplying the observed ΔQ, given in thermal units (*M. K. S.*) in Table 4, by 4186, thereby converting thermal into mechanical units. The sums for cols. 5 and 6 (Table 5), namely, 26664 and 26735, are almost the same, so that, as a whole, the two methods seem to be equally valid; but there is a great difference in the distribution in altitude. We have seen that the observed ΔQ is much the larger of the two near the surface. In spite of the different methods employed in obtaining Tables 4 and 5, both agree on the latter point. Such a notable divergence from the theory which is supposed to express internal interchange of heat requires explanation. Apparently, the thermodynamic theory (which is obviously incomplete and provisional), does not

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per sec.,^{2} or one million times the pressure which will produce in one second an acceleration of the velocity of a mass of one gram by one centimeter per second, when exercised over an area of one square centimeter. A pressure of 10^6 dynes/cm.^{2} sec.^{2} is equal to 10^{10} dynes/m.^{2} sec.^{2} = 10^5 M. K. S. force units/m.^{2} sec.,^{2} since the M. K. S. unit pressure is the pressure produced by gravity acting on a mass of 1 kilogram. But the acceleration of gravity (M. K. S.) is 1/100 of its numerical value in C. G. S. units, and there are 1/1000 as many kilograms as grams. Hence the M. K. S. pressure on a square meter is one one-hundred thousandth of the C. G. S. pressure on the same area.

* For the purpose of this comparison, which is to bring out the difference in vertical distribution of heat from absorbed radiation, the density has not been removed from ΔQ, and must therefore be included in the value from the thermodynamic computation in Table 4.

allow for the mechanical importation of moisture and surface heat into the lower layers of air, but it does indicate the greater freedom with which the radiation of the upper air escapes to space. The air near the earth's surface is laden with moisture and absorbs a great deal of the radiation from that surface, becoming heated thereby.* This heat becomes the motive power of the mechanism of convection. But the heat communicated to the air by absorption of surface radiation is not readily lost to space, owing to the obstruction imposed by the upper layers. Hence there is accumulation of heat in the lower layers of air, and while the potential radiant function of these layers (*K* in the Bigelow system) is large, comparatively little heat actually escapes from them to space, especially in moist summer weather. On the other hand, the upper air radiates much more freely to space, and while the upper air receives large accessions of heat from absorption of solar radiation, this heat is lost so readily by re-radiation to space that there is not enough left to satisfy the requirements of a theory which assumes that the thermal capacity of the air is replenished to its full limit. Evidently this is not the case in the isothermal layer.

The nearest approach to the thermal change appropriate to the adiabatic gradient is at 9 km. (as shown by the observed ΔQ, Table 4), indicating that at this point atmospheric radiation and absorption are nearly equal. Whether radiation exceeds absorption, or absorption exceeds radiation, makes no difference as regards this gradient; in either case the gradient is diminished. This has been shown by Gold and Harwood at the Winnipeg meeting of the British Association for the Advancement of Science, 1909; though I believe that Gold interchanges the functions when he makes the upper region above 7-9 km. one where absorption exceeds radiation, and the region below 7-9 km. one of reversed relation; whereas in my view the relation is just the other way. The increase of $Q_1—Q_0$ in the isothermal region shows that this layer is one where great absorption is taking place, but this does not forbid still greater radiation to space, and a partial replenishing from other modes than the absorbent one. On the contrary, there is small exchange of heat below 7-9 km.; but nevertheless, absorption exceeds radiation, because of the great opacity of the lower layers to radiant transmission, and this produces the summer maximum of temperature in the lower air.

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* The importance of this thermal effect has been recognized in comparing the vertical temperature gradient in our Rocky Mountain plateau from sea level to the surface of the ground, with that of a free-air adiabatic gradient. The former is only 37 per cent. of the free-air gradient, which signifies that the sun-heated ground heats the superposed air by day (by means of convection and terrestrial radiation) more than it cools the air at night through contact with a surface, cooled by nocturnal radiation.

PART II.

THE CONTROL OF ATMOSPHERIC THERMODYNAMICS BY THE MORE GENERAL NECESSITY OF A STATIC EQUILIBRIUM

Let us now pass from the actual shifting atmosphere to the consideration of an atmosphere in static equilibrium. If we compute the altitudes from the given pressures and temperatures by the common formula:

$$(6) \quad z' - z_0 = (1 + 0.00366\, t) \times \frac{78376}{9.810} \times 2.3026\, log_{10}\, P/P_0,$$

where z' is the computed altitude above sea level (z_0), t is the observed mean Centigrade temperature of the air column, 9.810 is the value of g for the mean latitude, and P, P_0, are observed pressures at the upper and sea-level stations; and if, instead of observed pressures, we use the computed pressures taken from the first pressure column in Bigelow's Table 2 of the article in the *American Journal of Science,* the highest altitudes (z'), computed in Table 6 by the above equation (6), are larger than those assigned by Bigelow (here given under z in the first column of Table 6); but the values of z' for the lower altitudes are usually smaller than z, being on the average 95m. too small below 10 km. The supposition that the discrepancy is instrumental would require an aneroid-lag in this region of about 8 mm. which, as we have seen, is excessive to the extent of several hundred per cent. Consequently, while there is some evidence of a small amount of lag in the aneroid reading with its maximum effect at 6 or 7 km., this alone will not remedy the discrepancy which arises chiefly from the adjustment of values to a hypothetical $Q_1 - Q_0$, graduated from an assumed limit of adiabatic decrement of temperature at the surface to the opposite extreme of upwardly increasing temperature-departures in the isothermal layer; whereas the result of this thermodynamic adjustment does not agree with the facts as shown by the observed ΔQ. In the computation of ΔQ (Table 4) from the observed temperature-gradient, we have to deal simply with a given sample of air in which there is a certain mean departure from the temperature given by adiabatic theory. We are not concerned at this point with the processes by which this temperature was attained, or with the thermodynamic relations between this and neighboring bodies of air. Consequently, we may treat this mass of air as being, at the moment, unvarying and as possessing the constant specific heat of an average sample of air of the given density, and may ask simply what quantity of heat, ΔQ, has been applied to the mass to produce the given temperature-departure.

Reversing the hypsometric formula, let us compute the pressure P at each 1000 m. of altitude by my equation (6) which presupposes complete static equilibrium, and compare with Bigelow's pressures (here denoted by P', Table 6) computed by his equation (6) from the temperatures,* on the assumption that the thermodynamic equation is literally fulfilled. We shall see that neither of these assumptions can be considered as exact; but the former is, on the whole, the one which is more nearly reliable.†

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* The accuracy of such computations as these, in which the true average temperature of the air column can be obtained from the balloon-record, exceeds that of most mountain work, where the usual assumption that the average temperature of the intervening air agrees with the mean of temperatures at upper and lower stations, is apt to be far from the truth.

† Langley, in his "Researches on Solar Heat" (p. 193), compared the differences of level between his camp on Mount Whitney and sea-level computed by various barometic formulas which have been proposed from time to time by different theorists. He points out as a fact worthy of notice that "the results from observations made at different times of the day by no means agree," even when computed by the same formula. Thus simultaneous observations reduced by Bessel's formula gave

from 11 observations, $\Delta z = 11{,}709 \pm 17$ feet at 8.15 a.m.
" " " $11{,}866 \pm 6$ " " 12.35 p.m.
" " " $11{,}563 \pm 6$ " " 8.15 p.m.

My equation (6) has been tested by levelings to the tops of mountains, and compared with results obtained by the use of the formula from long series of barometer readings which can be repeated under every condition of atmospheric stability or instability, and with the very high precision which the mercurial barometer gives. Its accuracy can not be impugned by the uncertain records of aneroids carried on sounding balloons, even though the temperatures recorded by the balloon are to preferred. While individual, simultaneous pairs of readings of the barometer at top and bottom of a mountain show considerable variations of ΔP, which are to be explained by the deviation of the intervening air-column from stability, yet in the long run, or if the survey is made over a sufficiently wide stretch of country at the base, these local deviations compensate, showing that in an average of a large number of observations, static equilibrium is fulfilled.

Computations by the thermodynamic formula from the results of balloon ascensions would probably have reduced these discrepancies very materially, but they would not have given the true altitude without further reduction.

From a large number of barometric reductions, Langley found the height of Whitney Peak above Lone Pine to be 10,762 feet, and of Lone Pine above sea-level, 3,883 feet, making the height of Mount Whitney, 14,645 feet by barometric observations which are wholly reduced by the hypsometric formula of Plantamour. Using an altitude which had been given him as the height of Lone Pine from railroad levelings, namely, 3,760 feet, Mr. Langley finally adopted 14,552 feet as the height of Mount Whitney; but since his Lone Pine camp was certainly somewhat higher than the railroad station, I shall adopt a mean of these heights, or 14,583 feet = 4,445 meters as being the best that barometric hypsometry could do in the given case. The height of the mountain has now been definitely determined by accurate levelings as 14,501 feet = 4,420 meters, whence the hypsometry was in error by +25m. Thermodynamics would have differed (and differed very consistently) from the hypsometry by about 100m., and from the truth by about −75m.

TABLE 6

COMPARISON OF HEIGHTS AND PRESSURES, COMPUTED BY THE ORDINARY HYPSOMETRIC FORMULA, AND BY THE THERMODYNAMIC THEORY

z	P*	P'†	$P—P'$	z'	$z'—z$
km.	M. K. S.	M. K. S.		m.	m.
18	8,017	7,851	+ 166	18,149	+149
17	9,317	9,141	+ 176	17,136	+136
16	10,862	10,983	− 121	15,921	− 79
15	12,630	12,819	− 189	14,893	−107
14	14,695	14,972	− 277	13,896	−104
13	17,122	17,017	+ 105	13,046	+ 46
12	19,950	19,942	+ 8	12,003	+ 3
11	23,250	23,341	− 91	10,971	− 29
10	27,023	27,198	− 175	9,951	− 49
9	31,300	31,606	− 306	8,925	− 75
8	36,101	36,509	− 408	7,912	− 88
7	41,462	41,972	− 510	6,903	− 97
6	47,453	48,053	− 600	5,899	−101
5	54,121	54,819	− 698	4,896	−104
4	61,548	62,353	− 805	3,894	−106
3	69,805	70,722	− 917	2,892	−108
2	78,968	80,030	−1062	1,889	−111
1	89,151	90,345	−1194	888	−112
0	100,419	100,419	0	0	0

* The values of P are computed by the formula

$$\log (P/P_0) = z \div \left\{ \frac{78376}{9.81} \times 2.3026 \times (1+0.00366t) \right\}.$$

† The column headed P' has been computed by Bigelow from the formula

$$P'/P_0 = (T/T_0)^{\frac{nk}{k-1}}.$$

As derived by Bigelow the pressures are made to depend entirely upon the observed temperatures and temperature-gradients. Any errors in the record of the thermograph and any deviations from theory, such as those which accompany condensation of moisture where latent heat is set free, are passed on into the pressures. Moreover, as will appear farther on, the pressure values in the upper layers have really been assumed. Professor Bigelow says of the gradient-ratio, *n*, that "the means here given in this [the isothermal] layer were found by omitting from the summation several of the results which are excessive and due to a small denominator in $n = (T_a - T_0)/T_1 - T_0$" (*op. cit.*, p. 524). Since both the pressures and the densities have been derived from the temperatures by equations where *n* enters in the exponent, the uncertainty of the adopted values of *n* affects all of the derivatives. It is perhaps legitimate to adopt such values of *n* as are consistent with a reasonable hypothesis, as being the best thing that can be done under the circumstances; but it should be clearly understood that in doing this, we have departed from the rigorous mathematical sequence which makes the thermodynamics of the textbooks pretty much an exercise in pure mathematics. Not being able to suggest any better way of meeting the difficulties arising from the observed vertical temperature-gradients, I have accepted Bigelow's solution with its resulting new dilemmas and have tried to find an explanation for the latter.

The general result of the comparisons in Table 6 is to show that there is an excess of the pressures (deduced from temperatures) in the lower atmosphere (P' exceeding P), due mainly to the kinetic energy of absorbed terrestrial radiation, and to a very slight extent attributable to the neglect of the upward pull of the vertical wind-gradient; and there are the first signs of a deficiency of pressure (deduced from temperature) in the upper air, where radiation to space is more rapid than heating of the air by absorption of solar radiation. If the sum of the partial pressures in certain layers exceeds what is permissible from the stand point of static equilibrium, it is because we have not considered the relative upward component of pressure in these layers, produced by the excess of heat in them above that which would be apportioned by the law of static equilibrium, and also the effect of transference between neighboring columns; and of course there must be compensation of any remaining residuals, either in some other part of the column, or in a neighboring region connected through the horizontal circulation. The last is very important as an equalizer; for while any local thermal anomaly in the atmosphere is very largely dissipated through radiation from individual molecules to their neighbors in the gradual step-by-step process which has been elucidated in my publications on atmospheric radiation, the local inequalities of *pressure* in certain air-masses are more immediately equalized, or rather they are averaged, in a general hydrodynamical equilibrium by being handed on to other air-masses at a distance through the intermediation of aerial viscosity, in the same way that the weight of an aeroplane is distributed over an immensely extended supporting mass of relatively quiet air by the continual viscous transmission of its locally concentrated momentum to the surrounding medium.

AN ARGUMENT FOR A HIGHER EXTENSION OF THE ATMOSPHERE WITH APPRECIABLE TEMPERATURES AND DENSITIES AT GREAT ALTITUDES

The upper atmospheric layers near elevation 100 km. are probably at a temperature but little above absolute zero. The temperature-gradient here must be very small, and upper isothermal layers at temperatures not far from the boiling points of the component gases, ending with one near the boiling point of hydrogen are not improbable. All efforts to draw the curves of meteorological data at these great altitudes must be little better than surmise. Rev. Mr. Davidson has pointed out that phosphorescence takes place more readily at pressures such as are usually supposed to exist at 58 to 90 km. altitudes. But phosphorescent meteor trails are sometimes observed at double these altitudes. This seems to confirm the supposition that the pressures are greater and the densities less at this height than has been inferred from the formula for pressure in an atmosphere of uniform composition. We are confident that the composition of the atmosphere undergoes at least one great change when it is seen that hydrogen eventually takes the predominant place held by oxygen and nitrogen in the lower air, and it is possible that still lighter gases, such as coronium, may extend the limits of the atmosphere to several hundred kilometers. Six hundred accurate measurements of the *lower* limit of the aurora, as observed from Bossekop and Store Korsnes, two stations 27.5 km. apart in a N.-to-S. line, gave Mr. C. Störmer values of 90 to 100 km., for this height with marked uniformity. The longer auroral streamers apparently reach a height four or five times as great as the arch from which they spring, and this evidence favors an atmosphere of at least 500 kilometers depth.

STABLE VERTICAL EQUILIBRIUM IS THE EXCEPTION. ITS ABSENCE LEADS TO COLUMNAR INTERCHANGES

We may sum up the evidence thus far given as follows: Stable equilibrium does not commonly exist above any given single point in the actual atmosphere which is continually shifting in an effort to attain an equilibrium that is seldom reached. A self-contained column of atmospheric height, protected from outside influences, would necessarily be in adiabatic equilibrium from top to bottom. When kinetic energy is being variously communicated to, or abstracted from, certain parts of the air column, the pressure at any level is made up partly of the gravitational pressure of the overlying masses, so far as the widely separated molecules of the air are able to communicate their momentum to one another and thence to the layer in question; but, in part, this pressure is independent of the masses and varies with their changing kinetic energy. Of two air columns having equal masses, the pressure of one will be increased if it absorbs more radiation than it emits, everything else remaining the same; while a column of air which radiates faster than it receives heat by its absorption of radiation will diminish in pressure under like circumstances. Thus discrepancies are inevitable between the results of thermodynamic computations and those resting upon the supposition that the atmosphere is in static equilibrium throughout any single vertical column. Nevertheless, the resulting pressures for the entire atmosphere must be equivalent to those of an adiabatic column, because in general a thermally produced excess of air pressure in one part of the air column is apt to be compensated by a deficiency of the same in another part. This we may know since, otherwise, the hypsometric formula founded on the supposition of average adiabatic equilibrium would not work, whereas it does answer fairly well to the requirements of practice.

SIR W. N. SHAW ON CROSS CURRENTS

Sir W. N. Shaw in a paper, *"Principia Atmospherica"*,* shows the need of considering the horizontal transposition of air at every level between adjacent parallel currents (or it may be only at special levels), in order that steady motion may endure. To make his thesis practicable for investigation, he adopts a treatment of the distribution of pressure and temperature between four quadrants of an atmospheric plot at successive kilometers of altitude, getting his data by means of pilot balloons. His conclusion that such intercommunication of neighboring vertical air-masses follows from the general equation of steady motion in certain cases, and that it explains several recognized laws of atmospheric movement, seems to be justified, and is in line with what has been shown above. I should wish, however, to take exception to a few of his remarks.

Dr. Shaw considers first the steady motion of a layer of air between the 1-km. and the 10-km. levels: "To provide for the additional air by increasing the height above the selected base-level, would result in altering the pressure; that mode of operation is therefore excluded by the condition of maintenance of the current as steady." But that is just what happens in nature, and therefore the assumed steady state is commonly not realized for more than a brief interval. For example: In order to supply air to a deficiency of air at some higher-altitude point in the northern hemisphere, a body of southern air starts to move northward. Retaining its rotary velocity around the earth's axis, this northward-moving mass comes into regions of smaller radius vector and ascends by its momentum, thereby piling up more air over the northern region and increasing the pressure, and thus tending to obliterate the original deficiency. Omitting the artificial requirement that the current shall be steady, which is adopted in order to give the argument a definite, workable mathematical form by making $\frac{d\,u}{d\,t} = o$, the south-to-north current gives access of atmosphere which should result eventually in downward flow, while the north-to-south current diminishes the pressure and requires outside supply, as in Shaw's Fig. 2; but the explanation differs from his by requiring vertical currents at *every* level, ***not excepting the isothermal layer.***

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* Published in *Proceedings Roy. Soc. Edinburgh*, Vol. 34, pp. 77–112, of which a reprint, to which I shall refer, appears in the *Monthly Weather Review* for April, 1914, pp. 196–209.

Dr. Shaw says (*op. cit.*, p. 208, col. 1): "Radiation is left out of account—whether rightly or wrongly, it is not possible at this stage to say." On the contrary, it seems to me to be demonstrated that radiation is a most important factor and the key to what goes on in the isothermal layer.*

"In the stratosphere from 11 kilometers upward it is colder in the high pressure than in the low pressure at the same level," says Dr. Shaw (*op. cit.*, p. 199, col. 1). To the question: "Why is this so?" I answer: Because the normal source of air supply at these high levels is furnished by ascending currents, that is, by south-to-north currents (as we have just seen); while the normal remedy for a surplus of air at the high levels is by descending currents which are associated with north-to-south movements. The south-to-north current which supplies the local deficiency in the high-level low, expands in ascending and is thereby cooled; but this tendency to lower temperature is completely over-ruled by the replenishing of the dry isothermal layer at the given point with moisture brought up from below. *The moisture acts as a powerful absorbent of solar radiation* and gives rise to a very large local increase of temperature throughout such regions of aqueous replenishment. Both the presence of the aqueous vapor and the fact of very large, short-period fluctuations of temperature in the isothermal layer are now established. They furnish important confirmation of the theory of the isothermal layer which I published several years ago. Layers of surface air, on the contrary, can not be supplied from below, if locally deficient, but must be fed by horizontal currents; and throughout the lower layers, the isothermal layer has a controlling influence; for if more solar radiation is absorbed in the upper air, less can penetrate to the lower layers to give them heat, whence there is a complementary opposition of upper and lower conditions in respect to intensity of absorbed sunshine.

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* See my paper: "Sky Radiation and the Isothermal Layer." *American Journal of Science*, Vol. 34, pp. 369–388, April, 1913.

The ascent of the isothermal surfaces in the isothermal layer in departing from the equator, and especially the great upward extension of the isothermal of 219° Abs. between latitudes 40° and 70° in Europe and North America, or in those parts of the northern hemisphere where the diminution of the radius vector per degree of northing is especially large, demonstrates that this over-running of moister air from more southerly latitudes (through the poleward turning of the planetary circulation in the higher latitudes on account of the earth's rotation) is continually going on. It is a legitimate inference from this great fact of the warming of the upper part of the isothermal layer in progressing from the equator to high latitudes, that this happens because air from the tropical high-pressure belts, moving polewards, retains its planetary momentum of rotation and overruns the lower layers. This transported air, as we have seen, carries moisture into the isothermal layer, and the moisture absorbs solar rays which gives heat.

Dr. Shaw does not think it necessary to consider these matters at present, but confines himself to demonstrating that the variation of the wind at every level, both in direction and velocity, must be considered in problems of atmospheric equilibrium. Professor Bigelow introduces the change of velocity into his general equation and stops there, because he is considering only a single isolated air column. My present contention is, that even a complete thermodynamic solution for an isolated air-column is inadequate in respect to the problem of atmospheric equilibrium in the adjacent columns forming a complex, and that connecting cross-currents must be studied.

Under his second "law of the computation of pressure," Dr. Shaw stipulates that in Clapeyron's equation, "R is a 'constant' which is altered by an alteration in the composition of the air *but not by other causes.*" On the contrary, R is altered if the containing walls are not impervious, so that there is leakage, and

this is the normal condition in the free air, where R necessarily varies, as Bigelow has demonstrated. Moreover, the proposition is not in accord with a legitimate deduction from one of the subordinate statements of the first "law of the relation of motion to pressure", namely, that ***"near the surface . . the friction due to obstacles and to the viscosity of the air prevents the steady state being reached."*** But the same causes permit deviation from the adiabatic gas-ratio immediately after leaving the surface, so that composition is not the sole criterion. Besides this, while there is eventual automatic readjustment of deviations from atmospheric equilibrium and an over-ruled effort to reach adiabatic conditions, it is necessary to add that, *far away* from the surface, the factor of aerial viscosity still permits the retention of wide variations from perfect equilibrium in not very distant air-masses, and that the restoration takes place through the slow process of viscous flow. Were it not for viscosity, the inequalities exhibited in wide deviations from the adiabatic law would be almost immediately equalized with explosive violence. This immediate equalization, however, does not take place. There are barriers to be overcome, which consist of quasi-walls of viscid air which divide up the available space into leaky compartments, where neither can the constancy predicated by the "gas law" of our laboratories be found, nor is there in the long run, or in wide averages for the sum of the dislocated parts, any marked deviation from automatic equilibrium. The deviations persist for a considerable time because of the cellular compartments. General equilibrium is nevertheless maintained in the wider view, because the compartments leak. Dr. Shaw and those meteorologists who think in terms of averages neglect the cellular structure of the atmosphere. Professor Bigelow, while treating a single isolated row of vertical compartments with elaborate precautions, is left with several dilemmas on his hands for lack of some way of following the processes which break down the barriers and prevent the isolation from being more than local, or temporary.

The diversity between the two points of view is ineradicable except through mutual modification and combination. Dr. Shaw thinks that "we may profitably turn our attention to the changes in the motion related to the varying distributions of pressure, and leave for the time being the endeavor to give a short answer to the question, 'What is the ultimate cause of any given distribution of pressure, with its attendant atmospheric motion?' " To one who looks upon thermodynamic causes as the "motive power" back of every atmospheric change, this seems like abandoning the essence of our subject. We must, perforce, sometimes take a superficial view of things for lack of intimate knowledge; but here this knowledge is pressing for recognition.

Dr. Shaw shows that "the variation [of the ratio of pressure to absolute temperature] from kilometer to kilometer depends generally on the ordinary diminution of pressure with height rather than any extraordinary distribution of temperature" (*op. cit.*, p. 204, col. 2). Variations in the vertical gradient of temperature which are far from extraordinary, have a notable effect on pressures, but without seriously affecting the *ratio* of pressure to temperature and its gradual change with increasing altitude. Pressures (P) being in millibars, and temperatures (T) in absolute Centigrade degrees, the ratio P/T diminishes from 3.55 at the ground to 1.18 at 10 km., and to 0.26 at 20 km., according to Shaw. The American continental values of this ratio in summer run lower than these values below 4 km. altitude by about 0.15 near the ground, and are a trifle higher (about 0.03 higher) in the isothermal region; while the agreement is good in the intermediate layers of air. The winter ratios, on the contrary, are higher than Shaw's by about 0.10 near the ground, and continue higher up to 4 km., but elsewhere agree very well with the sequence for the British Isles. These features, which are shown in Figs. 1 and 2, indicate a small seasonal variation in the above ratios. The substantial uniformity of this vertical φ (P, T) function everywhere bears testimony to the ultimate control of both air temperature and pressure by internal molecular radiation which tends to smooth out inequalities of temperature, and at the same time governs the thermal molecular movements which produce gaseous pressure. If the density were everywhere the same, the pressure would be a rectilinear function of the temperature. The increase of pressure, relatively to temperature, near the surface is due to the combined effect of the greater air density and the increased energy of motion of the air molecules, *i. e.*, the temperature function. Near the ground, the curve of summer observations (Fig. 1, dotted) falls below the mean, because the density of the air in the lower layers is diminished by the larger convection at that season. The winter curve (Fig 2) shows an opposite effect. A more minute and critical analysis is needed to bring to light the thermodynamic variations and their influence on the *P:T* ratio (*cf.* p. 55 below).

Average values of pressure and temperature have been used in Shaw's reductions, because pilot balloons give only wind directions and velocities at different heights, and no minute analysis can be attempted. "The error made is not fatal to the practical success of the calculation", when this concerns "the distribution of pressure and temperature in the upper air from the observations of structure represented by soundings with a pilot balloon" (*op. cit.*, p. 204). In like manner, the pressures computed from observed temperatures and temperature-gradients by Bigelow's formula which does not include all the minute variants, are not only not fatally in error, but they are more accurate than those given by the necessarily crude aneroid measures; and whatever the errors of the method may be, they do no harm as long as the object in view is the elucidation of thermal laws in respect to vertical variations; but when given a wider application, discrepancies appear.

My purpose in citing Dr. Shaw is not for the sake of substituting his method and abandoning the powerful thermal method, but for the valuable aid which he brings in solving some of the dilemmas of Bigelow's thermodynamic thesis, for which no thermodynamic aid is apparent.

Starting with the assumption that the normal air currents follow the isobars at every level in steady motion, Dr. Shaw's fundamental equation is that which represents the velocity of an air current on a rotating earth:

$$V = \gamma \div (2\,\omega\,\varrho\, sin\, \lambda)$$

where $V =$ wind-velocity, $\gamma =$ pressure-gradient, $\omega =$ angular velocity of the earth's rotation, $\varrho =$ density and $\lambda =$ latitude. "A west-to-east circulation or an east-to-west circulation in the upper air, once steady, will remain so, unless it is disturbed by changes of pressure distribution. But, on the contrary, when the air movement is from south to north or from north to south, or has any component which gives a motion across the circles of latitude, a change in sin λ has to be dealt with" (*Op. cit.*, p. 200, col. 1). "Persistence . . . requires the surplus of the adjacent southerly current and the *outflow from the northerly itself* both to be delivered to the northerly current in the upper layers in order that the proper temperature distribution may be obtained. Such a combination of circumstances may fairly be regarded as exceptional, and therefore the maintenance of a northerly current must be regarded as exceptional" (*Op. cit.*, p. 202, col. 1).

The above equation gives for the maintenance of a steady south-to-north wind from the one-kilometer level up to ten kilometers in mid latitude over a front of five degrees of longitude, the condition that there shall be a flow-off from east to west, in a lowest half kilometer of one and a half times the velocity of the upper current. The limitation of the flow to this layer is conventional. This under running east wind can only be continued while there is a region of high pressure on the eastern side and a low pressure on the western side to produce the east-to-west gradient of pressure slope, and the high-level northward current implies such a distribution of pressure with the main current held steady along the isobars, the flow-off needed for equilibrium being an exception to the rule; that is to say, in order that there may be a steady south-to-north current following the isobars *anywhere,* it is necessary that there shall be somewhere a cross current which moves, on the whole, with the gradient. The demonstration of the necessity and efficacy of cross-currents is complete. The "persistence, consequently, can not endure for any great length of time, and is even more precarious for a north-to-south current than for the opposite one, *except when juxtaposed beside a south-to-north current, the two lying on either side of a ridge of high pressure.*" The explanation given for the well-known persistence of this arrangement, namely, that here the momentum of a persistent eastward circulation is utilized to transfer air from the western to the eastern slope across the isobars, instead of with them as the general rule requires, is certainly attractive.

In accordance with these principles, the azimuths and velocities of air-currents observed at different levels up to ten kilometers by means of pilot balloons, are analyzed by Dr. Shaw into E.-W. and N.-S. components, with very consistent results which develop close relationships between the isobars and the isotherms at different levels (with occasional interruptions due to convection at particular levels); and this relation is obviously maintained by the local transverse air-currents, without which there could be no such thing as a steady meridional current through a large depth of atmosphere, nor the more common variation of velocities and wind-directions in superposed strata of air.

A circulation term is introduced in Bigelow's thermodynamic equation, but this is not treated in regard to azimuth for the reason that the vertical air-column is considered by itself, and not in its intimate relations with other columns. Dr. Shaw's treatment supplies this lack, but ignores the very necessary thermodynamics. A combination of the two modes is essential for a complete characterization of atmospheric movement.

For example, Dr. Shaw says: "Nothing is more noteworthy than the irregular variations of temperature difference as given by a pair of soundings with registering balloons, and the curious local irregularities of wind disclosed by pilot balloon ascents. Hitherto it has been customary, on quite general grounds, to regard them both as possibly due to uncertainties of observation. We now see that they may equally well be important evidence of complication in the structure of the atmosphere" (*Op. cit.*, p. 205, col. 2).* But the variations of the adiabatic departure and of the gas-factor, R, are equally important. Where the latter is approached, an alternative equation is used by Shaw (obtained from the preceding one, quoted above, by substituting

$p/R\theta$ for ϱ, and $\Delta P/L$ for γ) namely:

$$V = \frac{R}{2\,\omega \sin \lambda} \cdot \frac{\theta}{p} \cdot \frac{\Delta p}{L}$$

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* This admission by Dr. Shaw that there is thermal material in our present observations waiting to be incorporated into meteorological theory, is good as far as it goes; yet it is not entirely satisfactory, for surely the time has come to enter this field of radiant and thermal atmospherics.

where Δp is the pressure-difference in the distance L, "taken along the direction of the wind velocity V," θ is absolute temperature, p is the pressure in millibars, λ is latitude and $\omega =$ angular velocity of the earth's rotation (*Op. cit.*, p. 204, col. 1). Here the gas-factor, R, is taken constant. This has evidently been done out of deference to established, but erroneous custom. Strictly speaking, it would be proper to substitute variable values of R, determined by the observed adiabatic departures. If these have not been observed, average values of R could be used, just as has been done for the ratio of θ to p. Still, for his particular problem, since Shaw confines his attention to *steady* motion on one level, the precise value of R does not matter so much. The important thing is that R remains constant *in the given layer.* And this is evidently the reason why it has been possible to get consistent results in limited problems of horizontal circulation by using the adiabatic value of R. These problems have heretofore been the leading ones of meteorology, and hence $R =$ constant has become an accepted axiom. In the vertical relations, the variation of R is forced upon our attention, just as the deviation of wind-direction from a normal to the pressure gradient of the "steady motion" theorem is seen to be the rule. While it is proper to start from the ground with an adiabatic value of R, and to build up a wind system on the basis of a "steady motion" equation, the variations from these ideal simplicities are the all-important thing; and nature refuses to be forced into artificial molds.

We may note, in passing, that in spite of the explicit statement that pressure-gradients are given in millibars "per hundred kilometers" (*op. cit.*, p. 204, col. 1), this definition has not been followed in the computations. The prescript would make

$$K = \frac{R}{2\,\omega \sin \lambda} = 31.06,$$

for the latitude of 55° which is near that of Edinburgh where the paper was read. If the gradient were to be given for a *degree* of longitude at this latitude, K would be 19.79. A mean of the two, $K = 25.4$, has been used by Dr. Shaw in his Table 4 for no very obvious reason.

Special layers of the atmosphere at all altitudes show temperature-inversion, or turbulent motion. These are associated probably in every instance with some special absorption of radiation resulting in a disturbance of thermal equilibrium. Thermal and radiant problems can not be omitted without leaving out factors needed for even approximate fulfilment of atmospheric laws. Thus the isothermal layer exerts, as we have seen, a *thermal* control on movements in the lower atmosphere. The high-level data described by Professor Blair in successive numbers of the *Mount Weather Bulletin* demonstrate that this most extensive layer of the atmosphere is not an idle inconsequential part of the general atmospheric mechanism.

Dr. Shaw arrives at the conclusion that air-pressure is not solely dependent on gravity, and although he has not recognized the infringement of gravitational control which I have shown in what precedes on the thermodynamic principles, he names a cause which I shall add to the accelerations already enumerated which may act either in an upward or a downward direction, thus either counteracting, or aiding gravity. He says: "The ordinary values of the constant of gravitation assume the rotation to be that of the solid earth, and the acceleration of gravity upon air moving over the earth's surface is consequently different from that for calm air. Hence the air which forms part of a westerly wind is *specifically lighter* than air at the same temperature and pressure which is calm, and, on the other hand, air which forms part of an easterly wind is *specifically heavier*" (*Op. cit.*, p. 198, col. 1).

DEDUCTIONS WHICH MAY REASONABLY BE DERIVED FROM THE VERTICAL THERMAL GRADIENT OF THE ATMOSPHERE

Studies founded on the distribution of temperature have unfolded many of the secrets of the atmosphere. The curve of temperature which I published in the *American Journal of Science* for April, 1913 (Fig. 1, p. 372) is partly hypothetical in its extreme upper reaches, but the French sounding balloon ascension of December 11, 1911 (See *L'Astronomie*, July, 1913,

p. 328) tests it to the altitude of 35 km., and the approximately known limit of the hydrogen atmosphere requires that the temperature shall still be above absolute zero at altitudes at any rate approaching 150 km. The maximum temperature in the isothermal layer has been passed at about 30 km., and the layer can hardly extend above 50 km. without showing considerable cooling. A curve of double curvature connecting these separated portions is required. This being drawn by estimate in the part where observations are lacking, the area included between the great upward sweep of the temperature curve from 10 to 50 km. altitude, extended asymptotically to 100 km., and a similar extension of the curve observed between 0 and 10 km. (*op. cit.*, Fig. 1), represents the communication of a large amount of heat to the upper air. In the paper cited (p. 386) I found the ratio of these two areas to be 2.11 :1.00 From a consideration of the second differential of the free heat with respect to the height $(d^2Q/dz^2)_1$ Bigelow concludes in his "Meteorological Treatise" (p. 288) that "the isothermal region radiates 2.11 times as much heat as does the convectional region." If the identity of these independently determined ratios is only a coincidence, it is at least one of considerable significance. The heat in this upper region obviously comes from absorption of the *sun's* rays, because those *terrestrial* radiations which are capable of absorption by the air have already been sifted out and eliminated in passing through the lower layers. Absorption of the incoming rays by aqueous vapor and its products, and by ozone, oxygen and carbon dioxide in the order named, diminishes the intensity of solar radiation considerably before the rays reach the highest mountain tops. The heat communicated to the upper air through this absorption (which implies that a considerable fraction of solar radiant energy is used up in the highest atmosphere in processes of ionization) maintains a mean temperature-excess of something like 150° C. in the large body of air comprised in the isothermal region.

In Table 7 are given some approximate pressures (P) obtained by my equation (6) which neglects the change in the composition of the atmosphere at great heights, assuming further that the above mentioned temperature-curve applies. The mean temperature of the entire column up to the given height is stated on the absolute Centigrade scale and the gradient ($\Delta T/1000$ m.) in each step of 5 km. is also given. The column headed P' has been computed from the same temperatures by Bigelow's equation (6) without modification.

TABLE 7

HYPOTHETICAL PRESSURES (IN FORCE-UNITS, M. K. S.) AT GREAT ALTITUDES, COMPUTED FROM VERY'S ASSUMED TEMPERATURE CURVE; P BY THE HYPSOMETRICAL FORMULA, P' BY THE THERMODYNAMIC FORMULA

z	T at top	ΔT per 1000m	Mean T	M. K. S. force-units	
				P	P'
km.					
70	28.°5	−4.°5	181.°2	0.19	0.0044
60	73. 5	−5. 3	202. 3	4.0	5.8
55	100. 0	−8. 4	212. 6	14.6	42.5
50	142. 0	−8. 8	221. 6	45.1	176.7
45	186. 0	−3. 8	227. 9	118.1	504.0
40	205. 0	−3. 2	231. 8	276.3	1,208.4
35	221. 0	−0. 8	234. 4	611.3	2,696.0
30	225. 0	+0. 0	236. 2	1,309.9	5,800.0
25	225. 0	+0. 0	238. 4	2,791.1	5,800.0
20	225. 0			5,800.0	5,800.0

The values of P begin to be considerably too small at 60 km., because from this altitude upward, hydrogen becomes an increasingly prominent constituent of the atmosphere. Still more must the pressures, P', computed from the temperatures by Bigelow's formula, fail. They become totally erroneous at great altitudes, and are obviously equally erroneous in the isothermal region, since the pressure can not be constant through an ascent of 10 km., even if the temperature does not change. The aneroid may at least be trusted to settle this point. Some modification, or some criterion of limitation, must assuredly be made in a formula which leads to such discrepancies as these. They would constitute an outstanding meteorological puzzle which would challenge solution, were it not obvious that here the thermodynamic theory has exceeded its privileges through the abrogation of the limitations originally assumed in its derivation.

It is not meant that Professor Bigelow advocates the adoption of any such constant pressure values within the limits of a truly isothermal layer. Although the reader of the text of his paper will certainly suppose that the method of deriving the pressure through the temperature is given as a universal one, such is not the case; for an examination of the tables which he gives will show that their author abandons his theory whenever it leads to absurd results, as it does here, and without thinking it necessary to warn us of the fact, except in a very imperfect and casual way. He evidently holds his tenets "pragmatically." In his recent "Meteorological Treatise on the Circulation and Radiation in the Atmospheres of the Earth and of the Sun", where the thermodynamic formulas have been extended in an empirically modified form up to an assumed outer limit of the atmosphere, Professor Bigelow says (p. 365): "At first the *P* and ϱ curves were extended graphically, and from these values up to 40,000 meters approximate values of *T* were computed, carrying the *T* curve around the corner of the isothermal region into a very rapid gradient. These temperatures are necessary to continue the *P*, ϱ, curvatures at that elevation. Then all the other terms were computed directly from *T* and the diagrams were extended." That is to say, the formulas are abandoned when they become unruly, but are resumed after rounding the corner of a difficulty. We need not deny that makeshift methods are sometimes necessary in meteorology, but let us avow them openly.

Whatever heat may be produced through absorption of the sun's rays within the isothermal layer is devoted to expanding the air and maintaining its temperature in spite of a considerable radiation to space; but the density at successive levels follows very closely the radiant potential, and thus the relation between density (and consequent pressure) and temperature is not quite the same in the region where atmospheric radiation begins to escape directly to space, as that which has been found in the lower air. The smaller viscosity of the colder air at great elevations favors greater freedom of motion; but this slight increase in the freedom of motion appears to be more than overcome by the slowness of the gradual change in the vertical thermal gradient which takes place where the isothermal layer reaches its upper limit; and the small values of the thermal gradient probably account for the absence of convectional overturning in large volumes of relatively warm air which constitute this layer. The fact that here a part of the loss of radiant potential results from actual loss of radiation to space in addition to that loss by internal radiation which is elsewhere associated with the vertical gradient of density, is the principal distinction of the isothermal layer.

In Fig. 3 is shown the proportionality of density (ϱ) to radiant potentiality (K) in a vertical section of the atmosphere between 1,000 and 19,000 meters. Abscissas denote air-densities in kilograms per cubic meter, and ordinates are potential radiant energies per 1,000 meters. Circles are for latitude 0° and crosses for latitude 50° N. The data are from averages given by Bigelow in Tables 31 and 40 of his "Meteorological Treatise." The deviation from uniform proportionality between these quantities, or from the straight line *AC*, is largest at the bottom of the isothermal layer at about 10,000 meters, and the departures are obviously associated with the causes which have produced this layer. The density of the air is too large in the middle altitudes where the temperature reaches a minimum value; but below 10,000 m. the observations may be represented by the straight line *AB* which corresponds to a change of radiant potential of 9,230 units per change of 0.1 in the density; while above 10,000 m. the observations agree with the straight line *BC*, corresponding to 7,580 units of loss of radiant potential for a diminution of 0.1 in the density. This slower rate of change of K in the upper levels is due to the increase in the second differential of Q in these same levels; and this, in turn, is a consequence of the local addition of heat to the air by absorption of solar radiation which partly relieves the expenditure of energy required to maintain the expanded condition of the air.

The general control of the air-density by the radiant potential has its further effect in determining the average ratio of pressure to absolute temperature which has been noticed (*ante*, p. 47; and Figs. 1 and 2).

Ferrel* proposed to treat the pressures of the several ingredients of the atmosphere separately; but as quite erroneous temperatures were at that time attributed to the upper air, his examples are not especially instructive. We are certain that the pressures in the upper levels must be much increased above the values of P just given to allow for changed composition. Dr. Humphreys who has repeated Ferrel's computation with modern improvements, gets such pressures as these:

54.1 , instead of 45.1 at 50 km. (Table 7);
12.5 , „ „ 4.0 „ 60 „ „ ;
3.65, „ „ 0.19 „ 70 „ „ ;

and these should no doubt be accepted as very nearly correct.†

STATIC EQUILIBRIUM, OR ITS EQUIVALENT, IN SPITE OF LOCAL LAPSES, IS A GENERAL FACT WHICH DOMINATES ATMOSPHERIC THERMODYNAMICS

While the actual pressures at any given level may deviate somewhat from those given by the theory of static equilibrium, because of interference with the regular progression through those thermal changes which are considered in the thermodynamic equations, neither the height of the atmosphere nor its approximation as a whole to the condition of static equilibrium can vary through more than a moderate range, and the application of thermodynamic theory to the atmosphere is limited by this larger requirement. It is improbable, however, that any association of atmospheric data of pressure, temperature, etc., can be in complete thermodynamic equilibrium in one and the same air-column. Just as the weight of a rapidly moving aerodrome which takes hold of the air by means of aerial viscosity,* is distributed over a wide area, the gravitational pressure eventually resting upon the earth's surface, but only after innumerable transferences of momentum;—so the processes which produce the atmospheric thermodynamic relations connect widely separated regions of air, and the whole story is not given by a single line of atmospheric soundings, even supposing that this sounding could be made instantaneously, which it never is.

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* "Recent Advances in Meteorology." Appendix 71 to the Annual Report of the Chief Signal Officer, U. S. A., for 1885, p. 37.

† Professor Bigelow, in extrapolating from his own curve of pressure by the empirical method noted above, and ignoring the modifications which must be made on account of changed composition, obtains in his "Meteorological Treatise" (Table 85): for 50 km. 21.371, 60 km., 1.0021, 70 km. .018766, which are erroneous in spite of their five significant figures.

Extreme variations from stability in widely separated regions give rise to counterpartal movements—a juxtaposition of cyclone and anticyclone—and if the contrasts are great, the tempest becomes violent. The violations of thermodynamic equilibrium are concomitant with others which are hydrodynamic. The variations from static equilibrium, shown by local thermodynamic relations, are true in their limited connection, but they fail to grasp the entire chain of cause and effect, and can not be taken as an abrogation of a wider and far-reaching static equilibrium on which these local variations are superimposed. If the atmosphere over a portion of the earth's surface is heated without experiencing any other change, the air expands upward, and in being elevated, work is performed against gravity at the expense of pressure. Consequently, the pressure over such a heated area must diminish. This explains how it is that, although the addition of water vapor from surface evaporation must, if considered by itself, increase the mass and therefore the pressure of the atmospheric region thus receiving accession, yet, since aqueous vapor is a powerful absorbent of radiation, this region may be heated by radiant absorption to such an extent as to more than compensate, and the region becomes one of excessive heat and diminished pressure through thermal expansion of its air.

From the foregoing principles it results that, in general, it is not possible to obtain from the thermodynamic formulas applied to a single section, a consistent series of pressure-differences in a given vertical which shall be so regularly disposed and intimately related that their sum can be said to exactly equal the surface pressure; but such series of pressure-differences according to altitude are commonly dislocated and variously transferred to other, and often quite distant verticals. Only under conditions of complete uniformity, either of motion or of rest, can static equilibrium be secured in a single vertical column, taken by itself; and yet, notwithstanding the modifications of pressure by the transference of air masses in wind veins, by the exhibition of latent heat through condensation of aqueous vapor, or by expansion through acquirement of heat by absorption of radiation, still the combined pressures from many vertical columns taken over a great range of country must, if sufficiently extensive, form one system, equivalent to a static equilibrium, because excesses are always balanced by deficiencies somewhere else.

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* Cf. Frank W. Very. "The Rôle of Viscosity in Air Support of a Moving Aeroplane." *Technology Quarterly*, Vol. XXI, No. 4, p. 490, December, 1908.

The comparisons made in this paper reveal a few discrepancies between the results predicted on the thermodynamic principles (as modified to adapt them to atmospheric conditions) and the observed data; but it appears probable that this is mainly due to the confinement of the data to imperfectly observed vertical sections of the atmosphere up to 18 km., whereas the redistribution of local accumulations or deficiencies of energy by lateral movements and along lateral as well as vertical gradients, is required in the struggle to attain static equilibrium; and knowledge of these missing data is demanded before there can be a completely consistent thermodynamics of the atmosphere. When all of the facts are assembled, there should be no disagreement between either a thermodynamic or a hydrodynamic solution of the problem. If the limitations of atmospheric theory through lack of essential data and the great need of further development of knowledge to supply this lack are evident, the recognition of these imperfections need not detract from the merit which attaches to the application of the modified thermodynamic equations to some of the problems of the atmosphere by Professor Bigelow. The far-reaching consequences of this new departure will become more evident as the gaps in our knowledge are filled.

While Bigelow was right in insisting upon the use of his "non-adiabatic" equations in the immediate thermodynamic problem with which he was dealing and for which, as he shows, the adiabatic equations employed by Bjerknes and others are entirely inadequate, it is nevertheless true that in the wider hydrodynamic problems of the atmosphere which Bjerknes has elaborated in his masterly treatise, an average adiabatic assumption is often sufficient. I have shown how these discrepancies may be reconciled.

The interchange of hot and cold air between tropic and polar regions in alternating northward and southward movements is the final cause of the system of cyclones and anti-cyclones which circulates around each pole; but the immediate cause of the wind systems is the distribution of pressure. The effort to attain static equilibrium tends to obliterate extremes of pressure, except as this is hindered by the broad areas of moist air which cover the oceans. These can not be permanently displaced, and they are therefore barriers which compel the deviation of those atmospheric movements which make for the restoration of equilibrium. The polar and considerable portions of the temperate zones are covered by areas of low barometer of irregular shape which are bounded by the tropical belts of high barometer. Considering only the north polar low, it may usually be divided into from four to seven or eight subordinate cyclonic areas, between which protrude tongues of high barometer from the tropical belt.

If there were perfect symmetry, there would be a regular succession of equi-spaced wave-crests and troughs of this interlocking system. Something of this sort apparently does take place along the Antarctic border. Each high obviously tends to coalesce with and obliterate its neighboring lows; but thermal differences equally tend to restore and resuscitate the system. The alternating land and oceanic barriers of the northern hemisphere prevent the attainment of symmetrical regularity and interfere with the application of rules which might otherwise be devised from the principle of static equilibrium as applied to a system of wave-crests. Something, however, may possibly be done in this way in spite of this drawback.

THE REMEDY FOR REMAINING DISCREPANCIES IS A MORE COMPLETE COMPARISON OF NUMEROUS VERTICAL ATMOSPHERIC SECTIONS IN THEIR TRANSVERSAL RELATIONS

To study the hydrodynamic flow of the atmosphere in a whole hemisphere, even for a single day, would be a work of great labor, and one for which, as yet, the data are insufficient; but since the hydrodynamic movements result from a distribution of pressures which must fulfil the condition that there shall be the equivalent of hydrostatic equilibrium for the world as a whole, or approximately for either the northern or the southern hemisphere, we can test the reality of a world-wide hydrostatic equilibrium by such observations as already exist and in this way may confirm the rational prevision that, though the results of computations founded on partial thermodynamic, or hydrodynamic data may appear to contradict the demands of static theory, the latter must nevertheless be in full control over the atmospheric movements of the entire earth, and this will be shown whenever the observations extend through a sufficiently wide area.

A correlation between widely separated areas of the earth's surface appears to be one of the necessary consequences of a departure from uniformity. Equilibrium requires that if positive forces prevail over an extensive area, there must be some correlated area over which negative forces hold sway. A digression into a distant field of science will illustrate this point.

It is known from geology that a large part of the Mississippi basin and adjacent terranes was oceanic during the earlier geologic ages, while enormous deposits of sediments in New York and Pennsylvania appear to have come from the northeast, indicating that there was a large continental mass in this direction. The sinking of this continent beneath the ocean has apparently proceeded *pari passu* with the gradual elevation of the south-central United States from the depths of the sea. Again, we find the greatest depressions of the North Atlantic Ocean alongside of the Antilles which are even now rising out of the oceanic depths, constituting the beginning of a new land. Similarly, the greatest depth of the Pacific Ocean closely parallels the coast of rising

Japan, a land still in the throes of active volcanic upbuilding. It is fair to suppose that these juxtaposed areas of opposite tendency are correlated. If deep-lying levels of the earth's crust can in any way become viscid, as Joly supposes may be the case through heat developed in radio-active material, the intrusion of such softened rock beneath adjacent strata may be the cause of extensive changes of level, even of almost continental magnitude, and this in spite of an average isostasy at any given epoch, which changes with extraordinary slowness.

Similarly by means of the viscosity of the air, deviations from a regular gradient of pressure at any level may be transferred horizontally to remote localities on the same, or on some not very different level. This is accomplished by horizontal veins of moving air which transfer a particular body of air having an excess or deficiency of temperature or humidity, on which the local pressure depends, to a region where it may be equilibrated. If the air were a perfect fluid, its adjustments would take place with such speed as molecular motion gives; but through internal friction these motions are transformed into an intricate tangle of vortices. The maze of intertwined vortex filaments has sufficient cohesion to be transported as one piece to distant regions where its excess or deficiency of pressure is gradually overcome by the slow process of admixture with other masses equally entangled. Consequently, considerable deviations from uniformity arise in pressure gradients.

ILLUSTRATION OF THE MORE GENERAL HYDRODYNAMIC EQUILIBRIUM OF THE ATMOSPHERE OVER A HEMISPHERE

As an illustration of the actual conditions to be dealt with in meteorology, let us take the Weather Map of the Northern Hemisphere for January 5, 1914, published by the U. S. Weather Bureau. The adopted standard of normal pressure is 760 mm. = 1013 millibars. The nearest isobar to this on the map is 1015 millibars, and this isobar is here adopted as the limiting one between highs and lows. The lows will slightly exceed the highs on account of this small discrepancy.

The Polar and North Temperate area of low barometer on this date is circumscribed by the isobar of 1015 millibars on all sides, except in the region of the east and central Pacific where the tropical ring of high barometer is completely severed. The polar area may be divided by the meridians, 90° W., 30° W., 30° E., and 175° W., into five principal cyclonic areas, namely, a west Atlantic low (L_1), a North Sea low (L_2), a Siberian low (L_3), a Kamchatkan low (L_4), and an Alaskan low (L_5). The last extends southward and covers a wide area of the Pacific Ocean, apparently balancing the great development of high barometer over north-central Asia.

For purposes of discrimination, I shall arbitrarily divide the tropical belt of high pressure into five areas by the meridians 70° W., 20° E., 70° E., 140° E. To reach the greatest accuracy in the plotting of areas, it would be necessary to redraw the map on an equal-surface projection; but since, for lack of observations, the area of tropical high barometer is left open on the south, no advantage would be gained by this course, and as a compromise between the probable extension of the area to something like latitude 10°, and the contraction of this part of the map which an equal-surface projection would give, I shall again arbitrarily limit the area on the map by the parallel of 20° N. With these limits, the complete area of each isobaric curve, taken as if the others did not exist, multiplied by the step interval to the next isobaric curve (5 millibars) gives a number which is nearly proportional to the hydrodynamic intensity included in this isobar, since the positive errors are nearly the same as the negative in the entire series from high to low; and the sums of the positive and negative areas (multiplied by a constant) constitute the hydrodynamic intensities (I) of the given highs and lows. The accuracy of the observations is not great enough to require any closer approximation than this. The mean latitude and longitude of each high and low may be found from the center of gravity of the system. On the given date, the mean latitude of all the lows was 50° N., and that of the highs 37° N. Consequently, the entire system may be referred to the parallel of 48°.

TABLE 8

RELATIVE INTENSITIES OF CYCLONES AND ANTICYCLONES

Lows	L_1	L_2	L_3	L_4	L_5
λ	52.°5W.	2.°5E.	61.°E.	151.°0E.	141.°0W.
φ	52. 5N.	64. 0N.	66. 0N.	62. 0N.	51. 0N.
$\Delta \varphi$	+3.°5	+16.°0°	+18.°0	+14.°0	+3.°0
I	−2.335	−2.538	−2.184	−1.117	−2.615
Highs	H_1	H_2	H_3	H_4	H_5
λ	96.°0W.	12.°5W.	43.°0E.	104.°0E.	165.°0E.
φ	41. 0N.	35. 0N.	33. 0N.	40. 0N.	37. 0N.
$\Delta \varphi$	−7.°0	−13.°0	−15.°0	−8.°0	−11.°0
I	+2.089	+1.655	+1.468	+2.840	+0.965

There are two regions of greatest intensity and opposite sign, namely, the Alaskan low, L_5, and the central Asian high, H_4, which coincide very nearly with two great permanent centers of action, and whose activities dominate the two most extensive areas among the ten selected. These areas, whose centers are separated by 115° of longitude, may be considered counterparts, and though so far apart, they represent closely matched components of this extensive balanced system. The other highs and lows are approximately matched by taking pairs which are in juxtaposition:

$$H_1 = L_1;\ H_2 = L_2;\ H_3 = L_3;\ H_5 = L_4.$$

The last pair is the weakest, as $H_4 = L_5$ is the strongest of these combinations of oppositely contending forces. The sums of the intensities are $\Sigma L = 10.789$, $\Sigma H = 9.017$; Mean $L = 2.158$ Mean $H = 1.803$.

It would of course be desirable to use thinner isobaric sheets in making this summation, but this is not now practicable. Nevertheless, the result obtained by the present somewhat crude procedure will at least serve to confirm the reality of a static equilibrium for the entire surface of the globe. The small residual found for the northern hemisphere is not necessarily an error. It may very likely be matched by one of opposite sign for the southern hemisphere, and we have seen (p. 62) that the lows should slightly exceed in any case.

Again we reach the important conclusion that there are extensive regions of atmospheric inequality which are maintained intact for considerable durations, although gradually shifting their positions on the earth's surface; and just as, in the case of the long-continued duration of vertical inequalities, the separation of unbalanced forces implies that considerable portions of atmosphere can hold together as one piece through aerial viscosity, so in the greater masses there is an individuality and an acting together as one piece which requires the same viscous continuity on a still larger scale.

The interrelations of these great inequalities of pressure of the regional sort control the larger movements of the atmosphere. Thermal causes lie at the root of these pressure-inequalities, indeed, without heat there could be no atmosphere. Atmospheric thermodynamics and atmospheric hydrodynamics are related as cause and effect, but at present our knowledge of the thermodynamics is too local and limited in its application to make it practical for thermodynamics to supersede or to do more than supplement hydrodynamics in weather prediction, while the principle of ultimate static equilibrium controls all the rest.

These great atmospheric movements are like a system of vast waves which rise and fall in respect to a general level of static equilibrium, closely matched in rhythmic alternation and intensity. But these greatest waves could not be maintained, were it not for aerial viscosity, since otherwise they would immediately subside to the invariable static level; and as aerial viscosity is wholly produced by molecular interpenetration in the continual thermal activity of the air molecules, it is itself a thermodynamic phenomenon, and consequently the laws of thermodynamic and static-pressure equilibrium are so inextricably interwoven and made up of kinetics that they can not be separated. If we are obliged to consider them separately in our analysis, it is merely for lack of power to grasp the whole enormous complexity of the problem.

AN ILLUSTRATION FROM SOLAR PHYSICS

As a digression, but one which is quite germane to our subject, I will point out some solar examples of the power of radiant absorption to interfere with results which might otherwise be predicated on thermodynamic theory.

The great altitude of calcium vapor in the sun's atmosphere is probably due to its strong absorbent power in the spectral region of the very maximum of photospheric radiation. From the flash spectrum of the chromosphere obtained by S. A. Mitchell in the eclipse of 1905, taking heights from the strongest lines, calcium can be traced to a height of 14,000 km. and hydrogen to 8,500 km. St. John obtains for calcium and hydrogen in especially eruptive regions over sunspots, altitudes which are 11,000 km. higher than Mitchell's, namely, calcium = 25,000 km., hydrogen = 20,000 km., both of them completely confirming the fact that calcium ascends to higher altitudes than hydrogen in spite of larger atomic weight (calcium 40 times as heavy as hydrogen). The calcium is heaped up over special regions of the sun's surface in the irregularly distributed masses of the calcium floculi. These appear to be portions of the solar atmosphere which are especially rich in calcium, and therefore they are much heated by absorption of photospheric radiation. In consequence, they have strong ascensional force and attain altitudes which are much above those given by a theory which considers only the ordinary density of the vapor under normal temperature-conditions and omits the effect of absorption.

Dissociation (or ionization) is so nearly complete in the sun's atmosphere that it is much more transmissive than the atmosphere of the earth, where the absorbent substances are apt to be highly complex molecules—water vapor coalescing in certain atmospheric states to dihydrol and trihydrol, and ordinary oxygen (O_2) becoming O_3 and O_6. Traces of peroxide of hydrogen ($H_2 O_2$) and of the true carbonic acid ($H_2 CO_3$) are believed to be formed in the upper air, and various nitrogenous compounds exist in minute quantities. Consequently, the thousands of kilometers of the solar atmosphere absorb much less radiation than a few hundreds of kilometers of the earth's atmosphere. There is, however, a considerable depletion by the progressive scattering of the shorter waves, but even this is not as great as might be supposed from the great depth of the medium. The atmosphere which exists above the photosphere consists chiefly of *atoms* whose scattering power is small compared with that of air molecules. The absence from the sun's atmosphere of the complex ozone molecules with their excessive ultraviolet absorption, and the smaller loss of short waves by atomic scattering as compared with molecular scattering, permit the solar transmission of waves shorter than 0.3 μ which is the terrestrial limit; to which must be added that, though the sun's atmosphere absorbs quite a little of the photospheric ultra-violet radiation in its numerous absorption lines, the outer layers of each absorbent element selectively radiate these same rays, though at a lower temperature, and thus less effectively than the photosphere. The proof of this is, of course, the existence of the bright-line chromospheric and flash spectra,

In applying thermodynamic theory to the sun's atmosphere, there is considerable reversal of conditions. The source of radiation by which alone the sun's isothermal layer is heated, is *below,* instead of above as in this layer of our air, and the layers of incipient absorption of the sun's rays by the sun's own atmosphere are the denser layers, instead of the rarer ones. This makes a large difference between the layers of incipient absorption in the two cases; to which must be added that the radiant photospheric clouds, unlike the terrestrial clouds, float in an "isothermal" layer, for the endurance of practically the same surface temperature for millions of years must depend upon some process involving a physical constant, even though there is considerable range of *effective* temperature between top and bottom of the photospheric columnar structure, concerning which see below, page 73. The general, or average constancy, however, exists, not because there is little circulation in the photospheric, as in the earth's isothermal layer, but because it is thoroughly stirred by a most intense convection. Deeper down, this convective circulation either ceases, or becomes irregular and explosive at points of excessive thermal condensation. This is a necessary consequence of the increased viscosity of the solar subtance as the depth below the surface increases and the temperature gets higher. The deeper layers become viscid and unyielding to the pressure-gradients of the gravitational potential, except in that very slow and large-scale upwelling of the deeper solar substance in vast regional movements of long duration, which can only be compared to those age-long extravasations of semi-plastic material in the subterranean regions of our earth by which continents rise or fall. Stronger forces, such as those of atomic disintegration and reformation, are needed to take the place of ordinary convection in order to move these deep layers; and although such changes must be mainly hidden ones, it is possible that some relic of them remains in those spectral variations associated with high stellar temperatures on which Lockyer founds his hypothesis of "proto-elements". Moreover, the photospheric temperature is greater than the boiling points of known substances, and hence it is improbable that the photosphere is composed of clouds of liquid particles, maintained at the temperature of condensation from the vaporous state. On the contrary, ionization and molecular reformation are what probably take the place of evaporation and condensation under solar conditions. The solar clouds are therefore clouds of *molecules* floating in an atmosphere of *free ions*. The umbra of a sunspot is a region where the molecules are on the verge of forming. It therefore resembles a layer of nearly saturated air just before cloudy condensation of liquid water particles begins; and just as such layers of nearly saturated air are found to contain complex hydrol molecules which are the precursors of liquid water particles, so in the solar spots where the temperature has fallen, the beginning of molecular formation appears in band-spectra of titanium oxide and magnesium hydride.

SUPPLEMENTARY NOTE

RADIATION AS FLUX AND AS VOLUME ENERGY

IN an "Author's Abstract of the Meteorological Treatise, Atmospheric Circulation and Radiation, and the Astrophysical Treatise, The Sun's Radiation and Other Solar Phenomena" (New York: John Wiley and Sons; London: Chapman and Hall; 1917), Professor Bigelow does not mention the errors in the Meteorological Treatise which I pointed out in *Science* for December 3, 1915, but attempts to throw dust in the eyes of the scientific public by accusing me of a mistake which I have not made.

An equation numbered (24) in the "Abstract" gives radiation for an atmospheric layer in terms of the change in a quantity of heat $(Q_1 - Q_0)$ measured per unit volume of air, for which, since volume-density of heat, equivalent to the radiation, is to be used, the conversion-factor from the *M.K.S.* to the *C.G.S.* system is × 10. I have never used any other factor in making this conversion, although Professor Bigelow accuses me of "ascribing the factor 1000 to [his] volume data, which require the factor 10."

On page 18 of this pamphlet, Bigelow gives twelve different, but similar values of what he declares to be "the total effect of the solar radiation in the earth's atmosphere," of which eight are obtained from thermodynamic computations. They purport to be given in gram-calories per *square* centimeter per minute, that is, as equivalent radiant flux, for which the aforesaid conversion-factor is × 1000, as is correctly stated on page 8 of his "Abstract." But actually these numbers, which cluster around 4 and are derived from atmospheric formulas, have been obtained with the conversion-factor for volume-energy, and should be multiplied by 100 to give them as radiant flux. They have, in fact, only a remote connection with the "solar constant", instead of being reliable determinations of this quantity as this author asserts, and by no means prove that it is 4 cal./cm.^{2} min.; and they really represent the accumulated effect of many days of sunshine whose energy is stored up in the atmosphere, This is perfectly obvious, since, if the atmospheric effects were directly due to the sun's immediate radiant flux, the air temperature should fall to absolute zero at sundown; but the thermodynamic relations continue with little change by night as well as by day. Though originally derived in terms of volume-energy, there would be no harm in converting these quantities into equivalent radiant flux (except that it would be desirable to have the *average* flux, instead of a sum of fluxes, which, for this purpose, is meaningless), but this conversion has not been performed, notwithstanding the misleading label. This error was pointed out in my paper, "On the Solar Constant" (*American Journal of Science,* Vol. XXXIX, p. 203, February, 1915), and is alluded to here on page 29. As to the origin of this unfortunate misconception on the part of Professor Bigelow, I can give no more plausible suggestion than is contained in my footnote on pages 24-27, but that it is a misconception, I think there can not be the slightest doubt.

It is customary, though not obligatory, to state radiation as a flux per unit surface, viewing the radiation as the transition of a certain amount of energy from the thermal to the radiant mode, *at a surface.* The transition-surface has an inner and an outer face, and the transition may be considered on the thermal side in its relation to a volume of cooling matter; but outwardly it is viewed in respect to the volume of the light-bearing medium which would be filled by the radiation in the unit of time if there were no divergence of the rays, or 3×10^{10} cm.^{3} for each square centimeter of surface. There are therefore three modes of expression which must be equivalent, namely, volume-energy of heat in matter, radiation in the act, or radiant flux at a surface, and radiant propagation, that is, ethereal volume-energy, of radiation after it has escaped. Strictly speaking, only the last two can be called "radiation", but it has become customary to state them in terms of the equivalent loss of heat.*

Another example of the erroneous confounding of radiant flux with volume-energy, is to be found in a work entitled "Aether and Gravitation", by William G. Hooper, also published by Chapman and Hall. It is a somewhat pretentious tome, but

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*As noted *ante* in the footnote pp.24-27.

crammed with fallacies. On page 151, Mr. Hooper quotes a statement in Maxwell's "Electricity and Magnetism" (3d Edn., Vol. II, p. 441, art. 793) which says that "if in strong sunlight the energy of the light which falls on one square foot is 83.4 foot pounds per second, the mean energy in one cubic foot of sunlight is about 0.000 000 0882 of a foot pound, and the mean pressure on a square foot is 0.000 000 0882 of a pound weight." Mr. Hooper sees that the sun's flux of radiant energy of 83.4 ft. lbs./sec. through a square foot of section is quite a sizable quantity, and oblivious to the fact that Maxwell has divided by the velocity of light to get the *ethereal* volume-energy, that the action considered as a transformation is wholly superficial, and that the pressure is a vanishingly small effect, *except for minute particles,* he confounds (on p. 153) the radiant flux with the pressure of light and jumps to the conclusion that this is none other than the centrifugal force of Newton's law of gravitation which keeps the planets from falling into the sun!

The constant of radiation may be interpreted at pleasure, either as a volume-density of radiation, that is, of its equivalent heat in a volume of matter, or as a surface-flux. In the former case, its dimension is $\left\{\frac{ML^2}{T^2} \cdot \frac{1}{L^3}\right\} = \left\{\frac{M}{LT^2}\right\}$; but as flux through a surface into the ethereal form of energy, we must multiply by the velocity of light to give the distribution of the energy in the ether with the dimension $\left\{\frac{ML^2}{T^2} \cdot \frac{1}{L^3} \cdot \frac{L}{T}\right\} = \left\{\frac{M}{T^3}\right\}$ The latter is the most appropriate expression for the typical radiant mode.

It may be noted in passing that Bigelow's assertion that "the pyrheliometer registers only kinetic energy as temperature degrees of the thermometer, but fails to manifest the corresponding potential energy which is concealed in the atomic and molecular processes of the metallic apparatus", and that a large correction for unobserved "potential energy" (namely, 64.1%, a number derived from the kinetic theory of gases, which is not applicable to an instrument made of solid metal) must be added to the pyrheliometer readings on this account, is wholly unwarranted, because the gram-calory is defined as the amount of heat required to raise the temperature of 1 gram of water, *as indicated by a thermometer*, from 0° to 1° C. The thermometer is used only as an indicator. The definition says nothing of the distribution of the thermal energy, derived from the absorption of the radiation, between sensible and latent heat, but combines them into one total. Hence no correction is required on this account. Doubtless the pyrheliometer readings need certain positive corrections, but for a quite different reason.

It will also be well to note that Bigelow has used the term "solar constant" in an entirely new way to denote the constant of the sun's radiation *at the photosphere*, reduced to the earth's distance, or "5.85 gr. cal./cm.^{2} min." according to him, in place of the actual radiation received at that distance, This is liable to produce confusion. The method adopted for finding this quantity, which is inferred from spectro-bolometric observations between wave-lengths 1.50 μ and 2.50 μ, would give widely different and still greater values if obtained at longer wave-lengths in the infra-red; because the radiation of greater wave-lengths proceeds from increasingly deeper and hotter layers which probably resemble cloud in this respect, namely, in that they transmit the longer waves more freely than the shorter. They probably also have a species of cloudy structure as we shall see. Hence the effective temperature of the photosphere, as observed in its structural depths, is not any single value, but should be referred to a shorter wave-length than "1.50 μ", if we wish to know its superficial temperature. This makes necessary a considerable reduction of Bigelow's assumed "7655°."

In my paper, "The Solar Constant",* where I treated the problem of the derivation of a value of the superficial solar temperature from Langley's spectral energy-curves and the law of the movement of the maximum energy in wave-length with varying temperature, I first showed that the sun "does not radiate at any single temperature, but the photosphere being composed, as

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* *U. S. Weather Bureau Pub.*, No. 254, p. 22, 1901.

Langley has shown,* of brilliant granules, having a high emissive power, and occupying only one-fifth of the surface, while the background formed by the remaining four-fifths is relatively dull, it follows that the total photospheric radiation is made up of the radiant emission from particles at very different depths, and having doubtless a wide range of actual and effective temperature." Hence the unabsorbed spectral energy-curve must be the integration of many curves corresponding to many temperatures, and presumably none of them agreeing completely with that of the ideal black body. The result is that the ordinates (intensities) corresponding to the longer waves are raised above those of the black-body curve.

In my paper, "Solar Radiation",† applying my measurement of the depleting power of the solar atmosphere to a spectral energy-curve of the sun given by Abbot, I obtain 7146° Abs. C. for the effective temperature of the solar photosphere, stating, however, that both the hypothetical restoration of the unobserved ultra-violet and this computed temperature are liable to be underrated. While my value may be and probably is too small, I am of the opinion that Bigelow's "photospheric temperature" is overrated, and likewise his values of the solar constant, whether as referred to radiation outside the earth's atmosphere, or to radiation from a hypothetical sun supposed to be deprived of atmosphere.

In the solar thermodynamic computations, the pressure of the sun's atmosphere has been assumed by Bigelow as 6 atmospheres. This is much greater than Evershed finds it to be at the photosphere, and this will alter the thermodynamics materially. The assumption of the "Astrophysical Treatise" that the vanishing planes of the several solar gases are at altitudes above the photosphere which increase in inverse proportion to the atomic weight, gives from monatomic hydrogen, $Z_{(t=0)}=45{,}000$ km., for diatomic hydrogen, $Z_{(t=0)}=22{,}700$ km., and for monatomic calcium $Z_{(t=0)}=1{,}135$ km.; and no notice is taken of the fact that Mitchell's flash spectrum showed calcium at the height of 14,000 km., with hydrogen at 8,500 km. (*Astrophysical Journal,* Vol.

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* *American Jour. of Science*, Ser. 3, Vol. VII, p. 87, 1874.

† *American Jour. of Science*, Ser. 4, Vol. XXXVI, p. 613, December, 1913.

XXXVIII, pp. 438, 439, December, 1913), to which allusion is made on p. 66 of this paper, thus entirely reversing the order of heights demanded by the hypothesis. For these and other reasons, not much weight can be attached to the solar thermodynamics. As is shown (*ante*, p. 66) the exceptional buoyancy of solar calcium is due to its very strong absorption of photospheric radiation in a region of the spectrum where the latter has nearly its maximum efficiency. Similarly the outer limits of any other solar gas must be a function of both its density and its absorption of the solar radiant flux,
[Note added October 3, 1917]

SUMMARY

THE "radiant properties of the earth" must of course include those of the atmosphere; but more than this is requisite, because, for the largest part of the earth, namely, that included in the tropic and warm temperate zones, most of the radiation which the surface of the earth might emit at its high absolute temperature is prevented by the absorption of the aqueous constituent of the atmosphere. The earth radiates to the air and the air passes the energy along as internal radiation between its particles, and finally as air radiation to space. Thus, in the main, the "radiant properties of the earth" *are* those of its atmosphere.

Sir W. N. Shaw establishes his otherwise highly-to-be-commended paper—"*Principia Atmospherica*"—on a mechanical foundation which takes no account of the radiant properties of the air. "Radiation," he says, "is left out of account—whether rightly or wrongly, it is not possible at this stage to say." In spite of this assertion by so eminent an authority, it is not only possible to see the importance of atmospheric radiation as the chief factor among the earth's radiant properties, but it is imperative, if further advances are to be made in meteorology and in the branches of astrophysics which have an intimate connection with this nascent science, that we shall acknowledge the supreme importance of the new knowledge already at hand in regard to the radiant properties of the atmosphere.

Every one who studies physics learns that gases are subject to certain thermodynamic laws, and to a relation between pressure and temperature known as the principle of adiabatic expansion. But equally well every student of atmospherics knows that in a vertical section of the free air, examples of adiabatic temperature-gradients are seldom found. As a consequence, the established theorems of thermodynamics on which so much of engineering practice and physical theory has been built, have been neglected by meteorologists. It has remained for an American meteorologist and astrophysicist, Professor Frank H. Bigelow, to assure us that thermodynamic principles must henceforth be applied to meteorological problems. To this I would add, that quantitative measurements of the radiant properties of the atmosphere, which have been somewhat maltreated by Bigelow, can by no means be neglected.

The application of the thermodynamic equations to conditions in the free atmosphere for which they were never intended, and which require considerable modification of the original assumptions on which the thermodynamic theory was founded, can not give results of the same surety as those attending their ordinary use. Whoever imagines that this new departure will place meteorology on a sure foundation resembling that given to astronomy by the law of gravitation, will be disappointed. Nevertheless, the tentative application of thermodynamic principles to some of the problems of the atmosphere has already given valuable suggestions, some of which are capable of verification in more legitimate ways. This is especially true of the radiant properties of the atmosphere which require thermodynamics for their explanation. There can be no doubt that the principles of thermodynamics and of static equilibrium, both have their applications to the atmosphere, but the freedom of interchange between its constituent parts prevents a rigid mathematical continuity in the theory; and this is why meteorology remains one of the inexact sciences.

The following are a few of the leading facts made known by the study of atmospheric thermodynamics, facts which are to find their application in a truer knowledge of the thermal and radiant properties of the air:

1. The ordinary thermodynamic equations have been established with definite volumes of gas, enclosed in air-tight receptacles, and subjected to known changes of pressure or of temperature. In a section of the free air, we must consider definite volumes of air, but they are contained in leaky compartments, and the heat developed in descending masses, or abstracted in ascending ones to perform the work of expansion, is apt to have no such rigid amount as the adiabatic law requires. The cause is to be found in interference exercised by heat received, or

emitted under the form of radiation from or to outside masses, or through reception of outside masses of air of different temperature, transported from distant locations, and held together as one piece by viscosity, or, finally, by heat developed through friction in the reduction of wind velocity within the layer. By modifying some of the rigid rules of thermodynamics, they can be adapted to these new conditions, losing, it is true, something of their old precision, but giving useful approximations and an illuminating insight into the meaning of some of the obscure facts of atmospheric thermotics.

2. The kinetic theory of gases has recognized the effect of collisions between the moving molecules of the air in the partition of energy, but has not known much about the rapid equalization of thermal values among neighboring molecules through mutual radiation. After collisions there are redistributions of kinetic energy, half of which becomes latent internal motion, accompanied by vibrational distortions which produce gaseous radiation. Compared with the speed of light, the molecular motions are slow, and it is a relatively long time between collisions; but the radiation acts almost instantaneously and incessantly throughout the intervals between contacts. Collisions affect but a relatively small number of the molecules, but the molecular radiation penetrates to remote distances and affects immense numbers of molecules. Therefore it should not surprise us to learn that the internal molecular radiation has much to do with the establishment of average thermal conditions in a gas and that the interchange of kinetic energy at the moment of collision is not the sole equalizer, The thermal energy which gives air pressure appears to be really maintained by the internal radiation of the molecules. The curve of density in its variation with the height, agrees almost perfectly with the curve of change of radiant potential* in a vertical direction produced by intermolecular exchanges of radiation. This explains the excess of pressure and density in the lower air layers through nonfulfilment of the laws of adiabatic expansion. To a very small extent, the excess of

*Radiant potential, $K = U_1 - U_0/(v_1 - v_0)$, where U_1 and U_0 are internal molecular energies, and v_1, v_0 are volumes of unit mass at top and bottom of the layer.

pressure may be only apparent and attributable to aneroid-lag in an instrument carried by a rapidly rising sounding balloon; but in the main it comes from heat introduced by absorption of extraneous radiation. Since thermal losses must be equal to accessions of heat in the long run, the gravitational law of the equivalence between density and pressure is fulfilled in a general average, but there are wide local departures. Thus the larger positive departures of density in the lower air, which, if they persisted would soon limit the upward extension of the atmosphere, are compensated partly by a negative difference of gravitational potential in the isothermal layer, which does not become actual, because overturning of the widely separated air masses is prevented by viscosity. The remaining differences of pressure are due to imported heat and are not to be attributed to gravity.

3. There is great ionization in the higher levels of the atmosphere by the sun's ultra-violet rays. As a result, large quantities of solar radiation of short wave-length disappear totally in the upper air and are not recognized in the ordinary comparison of high-sun and low-sun observations. Besides this, as a secondary result of this ionization, there is great production of absorbent substances in the upper air, which profoundly modify the solar spectrum by absorption of the shorter waves, and are not without influence even in the extreme infra-red, The importance of telluric absorption bands in the violet and ultra-violet as modifiers of the sun's radiant energy is just beginning to be recognized. One consequence of this high-level violet absorption and of a further strong incipient absorption in the infra-red by water vapor—tenuous, but potent, throughout the isothermal layer—is the maintenance of a large depletion of solar radiation in this layer, while the ionization is probably most vigorous at still greater elevations; and since the local thermal energy thus produced in the isothermal layer must escape by transformation into air-radiation to space, there is an increased second differential of thermal interchange upon entering the isothermal layer which is highly significant. The determination of this fact by Bigelow from his thermodynamic equations is a strong check on the virtue of the computation. Though approaching the subject by entirely different paths, both Bige-

low and I were led to identical numerical results for this local production of heat in the isothermal region. In spite of their tenuous nature, the great expanses of the upper atmosphere exercise a considerable depletion on solar rays, and one which ought not to be overlooked in calculating the solar constant.

4. Still another instance of local production of heat in the atmosphere is that connected with the absorption of terrestrial radiation by the lower moisture-bearing layers of the air which seems to have escaped Bigelow's thermodynamic analysis, but is revealed by a different treatment of the data. This heat does not have much influence on the second differential of thermal interchange, computed through the temperature and pressure effects, apparently because the energy is used in another way, namely, in producing vertical motion of convection currents in the air. To recognize the remote effects of this absorption of terrestrial radiation by the lower atmosphere, they have to be considered in some detail.

In Table 5 (*ante.* p. 32), a small circulation term of little importance has been omitted and the loss of free heat in a given stratum, denoted by $(Q_1 - Q_0)'$ includes a small amount of heat produced by friction in a change of wind velocity between top and bottom of the layer. According to the thermodynamic theory, the loss of free heat is given in this case by the algebraic sum of a gravitational term and the ratio of change of pressure to density,

$$(Q_1 - Q_0)' = \{g\,(z_1 - z_0)\} - \{\,(P_1 - P_0)/\,\rho_{10}\,\},$$

where g is the acceleration of gravity and z_1, z_0, are the heights in meters at top and bottom of the layer, P_1 and P_0 are the corresponding pressures, and ρ_{10} is the mean density of the layer.

The pressures are computed from the observed absolute Centigrade temperatures, T_1, T_0, at top and bottom of the layer, by the equation

$$P_1/P_0 = (T_1/T_0)^{\frac{n\,k}{k-1}}$$

where n is the ratio of the adiabatic thermal gradient to the actual one, and k is the ratio of specific heat at constant pressure, divided by specific heat at constant volume for air, Starting with an observed surface pressure, the computation carries forward implicitly, from layer to layer, a record of all of the adiabatic departures. A similar equation gives the changes of density,

$$\varrho_1/\varrho_0 = (T_1/T_0)^{\frac{\kappa}{k-1}}$$

Taking layers of 1000 m. thickness, the first column gives the height in kilometers of the top of each layer; the next three columns give the pressure and density data in $M.\ K.\ S.$ units; the fifth column gives the thermodynamically computed values of $(Q_1 - Q_0)'$ which, being multiplied by the mass of 1 kilogram, are expressed as mechanical energy (joules/m.^{2} sec.).

For comparison with these thermodynamically computed values of the change in the energy of the free heat, the sixth column gives the imported heat, distinguished as $\Delta\, Q$, *directly obtained from the observed mean departure of the vertical temperature-gradient,* using the formulas,

$$\Delta\, Q = \tfrac{1}{2} \{ \Delta\, T - (-9^\circ.87) \}\, \varrho_{10} \times 0.238,$$

ϱ_{10} being the weight of a cubic meter of air of the average density of the layer, and 0.238 the specific heat of air referred to water. These values are given directly in kilogram calories per sq. meter per second, and are then converted into mechanical units by multiplying by 4186,

As the result of the comparison of an obviously imperfect thermodynamic theory with observation, there is found to be a great difference in the distribution in altitude, as shown by the differences in the last column of Table 5. The thermodynamically computed thermal change is largely in excess in the isothermal region, while the reverse is true near the surface. It seems evident that the thermodynamic theory does not allow for the mechanical importation of moisture and surface heat into the lower layers of air, though it does indicate the greater freedom with which the radiation of the upper air escapes to space. The air near the earth's surface is laden with moisture and absorbs a great deal of the radiation from that surface, becoming heated thereby. This heat becomes the motive power of the mechanism of convection. But the heat communicated to the air by absorption of surface radiation is not readily lost to space, owing to the obstruction imposed by the upper layers. Hence there is accumulation of heat in the lower layers of air. On the other hand, the upper air radiates much more freely to space, and while the upper air receives large accessions of heat from absorption of solar radiation, this heat is lost so readily by radiation to space that there is not enough left to satisfy the requirements of a theory which assumes that the thermal capacity of the air is replenished to its full limit.

The nearest approach to the thermal change appropriate to the gradient (as shown by the observed ΔQ) is at 9 km., indicating that at this point atmospheric radiation and absorption are nearly equal. The increase of the thermodynamically computed $(Q_1 - Q_0)'$ in the isothermal region shows that this layer is one where great absorption is taking place; but this does not forbid still greater radiation to space, and a partial replenishing from other modes than the absorbent one. On the contrary, there is small exchange of heat below 7-9 km.; but, nevertheless, absorption exceeds radiation, because of the great opacity of the lower layers to radiant transmission, and this gives the summer thermal maximum at the earth's surface.

All through the vertical column of air there are small, but sufficiently notable departures from the conditions which would be required by a strict conformity to static equilibrium. Thus, if we divide the atmosphere to a height of 18 km, into layers 1000 meters thick, and multiply the thickness of each layer by its mean density (ϱ_{10}), we find a discrepancy between the sum for the individual layers, and a total computed from surface conditions on adiabatic principles, getting

$$\Sigma\,(\varrho_{10} \times 1000) > \varrho_0 l_0 \times \frac{P_0}{Pn}$$

where ϱ_0 is the density of the air at the surface of the earth, P_0 is the observed pressure, Pn the normal barometric pressure, and l_0 the height of a homogeneous atmosphere; that is to say, a density-law which would be accurately fulfilled to the outer limit of the air, if the atmosphere were of uniform composition and adiabatic gradient, fails under actual non-adiabatic conditions. The failure, is really only apparent, and is caused by the neglect of minor forces which produce divergence. If we had entirely correct values of P and ϱ, the ratio, $(P_1 - P_0)/\varrho_{10}$, would not be constant at different altitudes, because adiabatic conditions almost never exist in the atmosphere, and at least two terms, namely, those for the effect of wind-variation and for thermal changes, must be introduced to make the equation balance. There are often small inversions of temperature at various levels, and in the isothermal layer is a large body of air in which temperature-inversion is perpetual. To attain equilibrium, this warmer air should rise and be replaced by colder air from a higher level. Consequently, the lower air is affected by an upward resultant gravitational potential which overcomes and compensates the downward acceleration of gravity upon a portion of its mass. But the overturning does not take place, because the unbalanced bodies of air are far apart, and their motion toward each other is prevented by aerial viscosity. The tendency of the lower uncompensated masses to rise may have a little influence on the differential circulation of the wind, since ascending velocities prevail in the wind circulation below the isothermal layer. Evidently, the correlative descending motions must occur at times of little wind, as in calms at the center of anticyclones. The vertical component of the circulation term derived from the wind velocity, as a rule, opposes gravity in the region below the isothermal layer, while the reverse holds true above this limit.

Pressures in the lower layers of the atmosphere are slightly diminished by the lifting effect of superposed winds of greater velocity, but a careful estimate of the magnitude of this effect shows that it is not competent to relieve local congestion. The excess of pressure in the lower air is almost wholly produced by heat introduced from the outside through absorption of radiation. Hence the pressure at any given level is not wholly controlled by gravitation, but thermal influences originating in the given locality are mainly responsible for the thermal divergences from adiabatic law and for excess of pressure. Differences of wind-velocity at top and bottom of a layer, due to friction, constitute in effect a horizontal vortex which tends to restore equilibrium; but in the isothermal layer, the horizontal air-movement is so small that this process is ineffective, and the great burden of maintaining a balance is thrown upon the thermal mechanism of molecular kinetics and radiation.

5. Finally we have the existence at every level of cross-currents of air which serve to equalize local abnormalities of pressure and temperature between neighboring vertical air columns, These have been well treated by Sir W. N. Shaw in his *"Principia Atmospherica"*, but without considering the very important radiant and thermal functions.

One of the most interesting of recent discoveries is the pole-ward elevation of the isotherms in the isothermal layer, and the concomitant variations in the curves of the second differential of the heat at high altitudes. They are caused by the lifting effect of the centrifugal force of the earth's rotation by which the otherwise almost quiescent isothermal layers are replenished with absorbent moisture, and fitted to capture the sun's rays, especially in middle and higher latitudes where this effect is greatest. The result is that, while cloud surfaces are good radiators as well as excellent reflectors of luminous solar rays, the isothermal layer is of all the strictly *aerial* atmospheric layers, the most efficient radiant one.

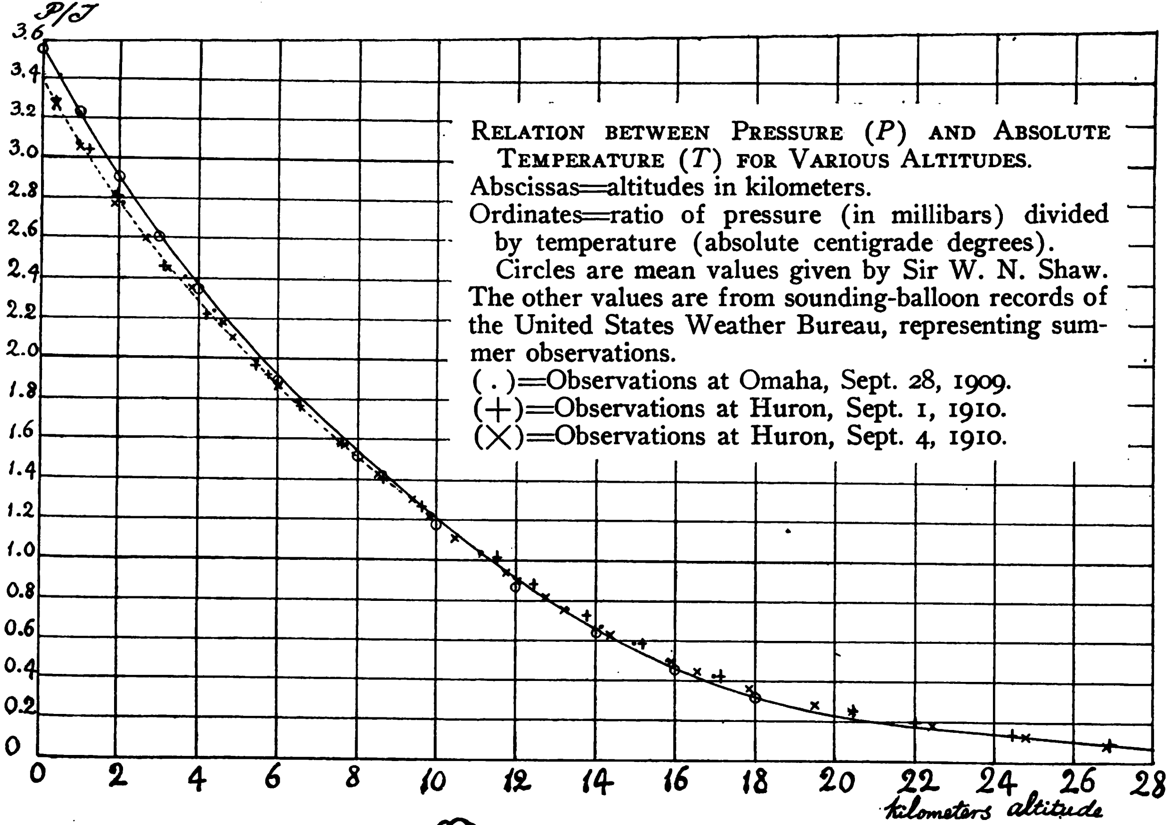
The (Bigelow) equation of equilibrium in the atmosphere on thermodynamic principles is

$$g\,(z_1 - z_0) = -\frac{P_1 - P_0}{\varrho_{10}} - \tfrac{1}{2}\,(q_1^2 - q_0^2) - (Q_1 - Q_0),$$

which expresses the general fact that in each layer of air between the heights z_0 and z_1., the downward pull of gravity, g, is compensated by the differential upward push of the molecular kinetic energy, without which the atmosphere would collapse. The pressures P_1 and P_0 at upper and lower limits of the layer and the mean density ϱ_{10} are to be computed from the observed temperatures by formulas which take account of the departures from adiabatic values. If there is no adiabatic departure, the last two terms which represent the change in the kinetic energy of circulation and the change of heat from without, do not exist, and the equation is much simplified; but very often a still more complicated equation is required, because this one says nothing of heat set free in condensation of moisture, Q representing the heat from radiant exchanges alone. Neither is there any azimuth term for the wind velocity, q, whereas the azimuthal relation of cross-currents is very important, as Shaw has shown. The equation is therefore incomplete, nor have we the requisite data to make it complete. Consequently, deviations from fact are often found in this as in every other form of "weather" prediction; but as an aid in the altitudinal study of the atmosphere, the equation is invaluable.

A thermodynamic reduction to C. G. Min. units shows that each air column of 1 sq. cm. section receives from the sun's rays an average thermal energy of something like 400 gram calories which represents the accumulation of between 2 and 3 days of sunshine. This is rendered possible by the slowness of the radiant interchange between the molecules on account of its innumerable repetitions and alternations between the radiant and the thermal states. This energy accumulated in the atmosphere is a reservoir of heat on which contributions are levied to ward off excessive or sudden thermal variations at the surface of the earth. As was predicted in my treatise on "Atmospheric Radiation", the greatest vicissitudes of temperature do not occur at the surface, but at high levels in the free air where the diurnal range of temperature often exceeds in the isothermal region anything ever known elsewhere. It is also worthy of note that the constant of atmospheric radiation which I established by laboratory experiments, namely, 0.000 000 06 radim per cubic cm. of air per degree Centigrade of temperature-excess, has been confirmed by the thermodynamic computations, along with the further fact that there is an upper limit of air radiation which never exceeds about one third of that of the ideal black body.

WESTWOOD ASTROPHYSICAL OBSERVATORY
August, 1916.



RELATION BETWEEN PRESSURE (P) AND ABSOLUTE TEMPERATURE (T) FOR VARIOUS ALTITUDES.

Abscissas=altitudes in kilometers.

Ordinates=ratio of pressure (in millibars) divided by temperature (absolute centigrade degrees).

Circles are mean values given by Sir W. N. Shaw. The other values are from sounding-balloon records of the United States Weather Bureau, representing summer observations.

(.)=Observations at Omaha, Sept. 28, 1909.
(+)=Observations at Huron, Sept. 1, 1910.
(×)=Observations at Huron, Sept. 4, 1910.

Fig. 1

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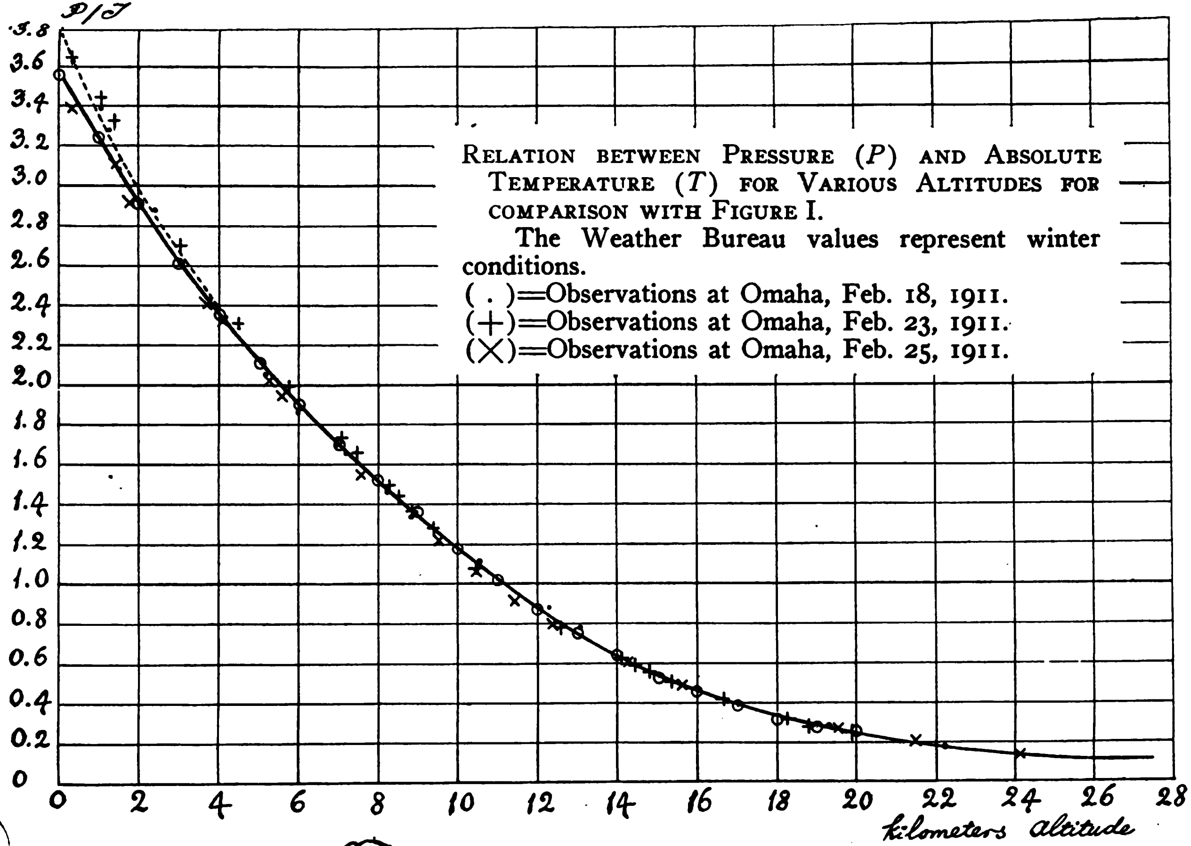

RELATION BETWEEN PRESSURE (P) AND ABSOLUTE TEMPERATURE (T) FOR VARIOUS ALTITUDES FOR COMPARISON WITH FIGURE I.

The Weather Bureau values represent winter conditions.

(.)=Observations at Omaha, Feb. 18, 1911.
(+)=Observations at Omaha, Feb. 23, 1911.
(×)=Observations at Omaha, Feb. 25, 1911.

Fig. 2

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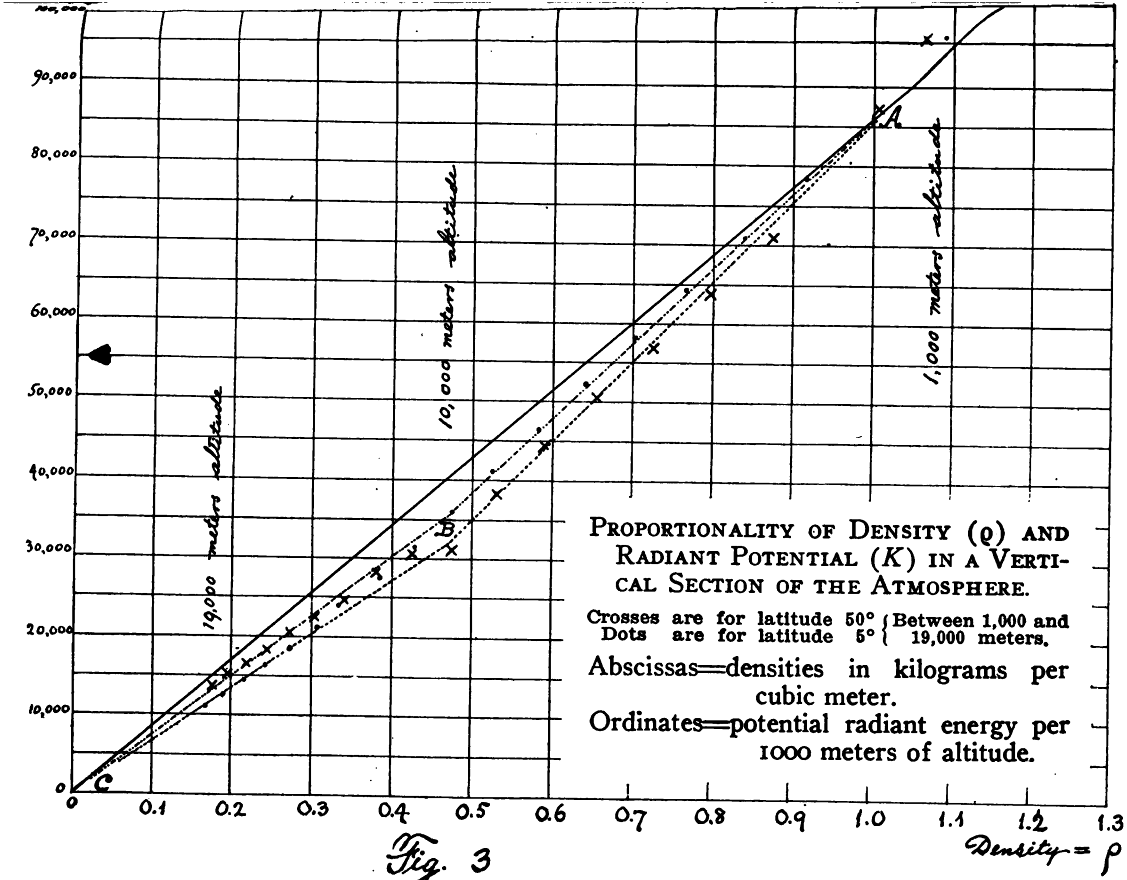

PROPORTIONALITY OF DENSITY (ρ) AND RADIANT POTENTIAL (K) IN A VERTICAL SECTION OF THE ATMOSPHERE.

Crosses are for latitude 50° { Between 1,000 and
Dots are for latitude 5° { 19,000 meters.

Abscissas=densities in kilograms per cubic meter.

Ordinates=potential radiant energy per 1000 meters of altitude.

Fig. 3

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